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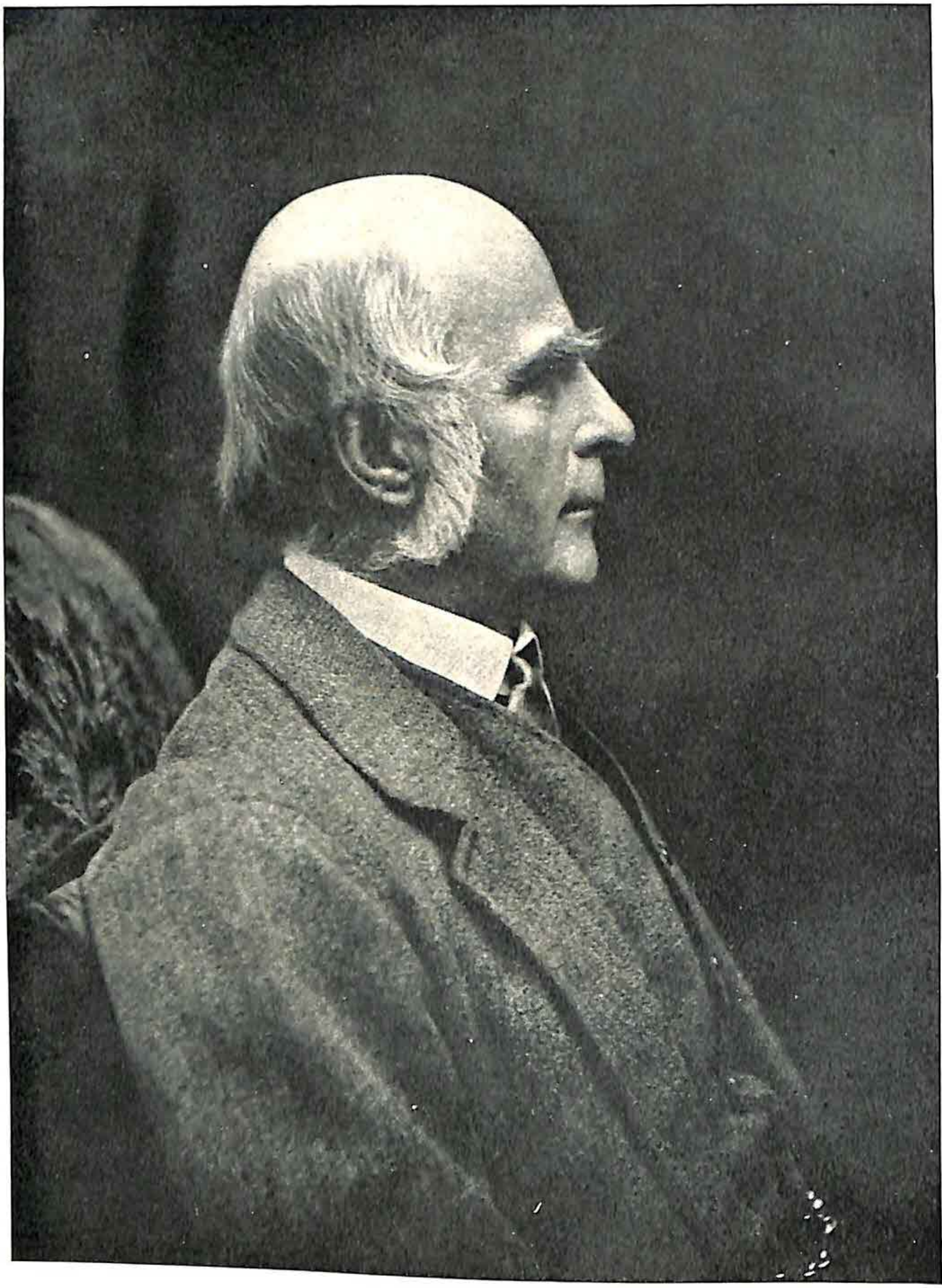

(Reproduced with the kind permission of Professor Egon Pearson)

FRANÇIS GALTON, aged 66

CALCUTTA
BUREAU OF ... RESEARCH

Vol. XV Part 1 The British Journal of Statistical Psychology May 1962

FRANCIS GALTON AND HIS CONTRIBUTIONS TO PSYCHOLOGY

By Cyril Burt[1]
University College, London

When I was a boy, my father, who was a country doctor, used to take me on his rounds to visit his patients. Of these by far the most impressive was the squire of a neighbouring village called Claverdon, a small Warwickshire hamlet, lying between Stratford-on-Avon and Birmingham.[2] The squire was an octogenarian named Darwin Galton. On his mother's side he was the grandson of Erasmus Darwin, physician, zoologist, botanist, and poet, and on his father's of a Birmingham Quaker who had made his money by manufacturing rifles. He himself was the eldest son in a family of nine. And, when he was fit, I was sometimes taken up to see him, and so met many other members of the same family, including the youngest, called Frank or Francis.

Francis Galton was one of the most distinguished-looking people I have ever known—tall, slim, neatly dressed, with a forehead like the dome of St. Paul's. My father, who was an ardent Darwinian, used to try to inspire me with intellectual ambitions of my own by telling me of the remarkable investigations carried out and published by this exceptionally brilliant member of an exceptionally brilliant family. I met him again as an undergraduate at Oxford, and later in London shortly before his death. He died in January, 1911, at Haslemere

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[1] Barely eighteen months ago psychologists were celebrating the hundredth anniversary of the publication of Fechner's *Elemente der Psychophysik*. The year 1860, which witnessed its appearance, also marked the birth of Francis Galton's interest in statistical methods as applied to the problems of individual differences. And in the following year, the fiftieth anniversary of Galton's death, the British Association and the British Psychological Society determined to commemorate his life and work by arranging a discussion of his many contributions to various sciences, and their value as assessed after half a century's progress. Professor Burt was accordingly invited to prepare a review of his psychological researches for the Annual Conference held at the beginning of the year; and the substance of that paper is presented in the following pages.

A shorter version was published in the Society's *Bulletin* for September 1961. The fuller account is printed here at the request of several readers who were unable to attend the meeting. We are grateful to the editor of the *Bulletin* for permission to repeat passages that appeared in its pages, and to Mr. J. C. Kenna, who keeps the Society's archives, for supplying some of the dates and incidental details. (*Editorial Note.*)

[2] Plate XXIX in volume I of the *Life and Letters* reproduces sketches of the mansion and church at Claverdon as they appeared during my boyhood. On Darwin Galton's death in 1903 Francis's nephew, Mr. Wheler Galton, inherited the Claverdon estate, and there (as readers of Pearson's biography will recall) collected a veritable museum of portraits and *personalia* relating to Francis and his various ancestors. My own family lived about four miles away in an old rambling house, where, as it happened, Mr. Wheler Galton had been born. This coincidence further strengthened the friendship between my family and the Galtons, particularly when Wheler Galton in his turn became my father's patient.

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(Surrey), in a house he had taken to escape the London winter; and his body now lies in the family vault in Claverdon churchyard. His biography, in four large quarto volumes, was compiled by his friend and disciple, Karl Pearson, my former colleague at University College.[1] It is largely to that work—supplemented by Galton's *Memories*, by my own recollections of him, and by the reminiscences of other members of his family—together with relevant articles in the *Dictionary of National Biography* and elsewhere, that I am indebted for the details summarized here. My aim, however, will be not so much to record or repeat the well established facts, but rather to interpret them.

I. Birth and Ancestry

Francis, the youngest son of Samuel Tertius Galton and Violetta Darwin, was born on February 16, 1822, at the Larches—a country house a mile and half from Birmingham where his father was partner in a large bank.[2] 1822 was by a curious coincidence the year in which another great hereditarian was born—the Abbé Mendel; and it is chiefly as the champion of heredity and the apostle of eugenics that we find Francis Galton depicted in Pearson's biography. He himself presents a most instructive example of inheritance; and the pedigree of his family, which Pearson traces back through a dozen or more generations, must be one of the most thorough genealogical studies in existence. Anyone who attempts to follow Francis's remarkable career will be struck by the fact that it is not in the least what one might have predicted from a knowledge of his closest relatives or his home environment. It was certainly not what his parents either anticipated or hoped for. Many of its unexpected turns can, I think, only be explained by his peculiar ancestry. Fortunately information about the various stocks from which he was descended is available in considerable detail; and a closer study may serve to illustrate in the concrete several of the debatable topics that have been raised in recent volumes of this *Journal*—the steady rise of highly intelligent stocks from humble beginnings, their marked tendency towards interbreeding or assortative mating, the occasional outbreeding, the intermittent appearance of distinctive traits both of intellect and of temperament, and finally the subtle influence of changing social and economic conditions and of the cumulative traditions that develop within such families.

The story of the Galtons is typical of the ancestral history of many eminent persons belonging to what today we should call the professional classes. Working backwards, we come first to the well-to-do father or grandfather—a successful man of business, then to the shrewd and industrious shopkeeper, and so eventually to the older yeomanry, the small farmer, and the ' rude forefathers of the hamlet ', sleeping beneath their scarcely legible tombstones in the village churchyard. And here our records begin to fail us. Yet often in one or two contributory branches there are strands which may carry bits of the pedigree back farther still to some knighted or ennobled kinsman about whom a good deal more is known. At every stage social mobility proves to be quite as important as social origin; and the investigator begins to recognize that yeoman, squire, baron, and king are all members of a single social hierarchy which is far more homogeneous and intermixed than our current ideas of class distinction are apt to suggest.

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[1] Karl Pearson, *The Life, Letters and Labours of Francis Galton*, Vol. I, 1914; Vol. II, 1924; Vols. III A and III B, 1930 (Cambridge University Press). Francis Galton, *Memories of My Life*, 1908 (Methuen).

[2] The house, or rather its predecessor, had belonged to Dr. Joseph Priestley, the discoverer of oxygen. Most of the original building and his laboratory had been almost completely wrecked by the Birmingham mob some thirty years before, on the second anniversary of the French Revolution. Priestley was a close friend of Francis's grandfather, Erasmus Darwin; and Samuel Tertius Galton had partly financed his experiments. Samuel Tertius took the house for a few years before buying Claverdon Leys; and it was in the meadows belonging to it that Charles Darwin and the Galton boys learnt to ride and to shoot.

On the paternal side, the oldest members of the Galton family seem to have been natives of the village of Galton, in the county of Dorset, farming or working on the land around. As commonly happened, the younger sons migrated first to neighbouring villages, and then to the larger towns, where some of them set up as retail traders. Later, when the industrial revolution arrived, they or their descendants became organizers of flourishing mercantile concerns. Thus, during the short reign of James II we find a certain Robert Galton starting a modest business as ' haberdasher of small wares ' at Bristol—the nearest large city, and in those days second in importance only to London.[1] Robert was followed a little later by several other members of the family. John Galton, Robert's brother, joined the Society of Friends; and in 1703 married Sarah Button, whose father had been imprisoned as a Quaker. Indeed, out of Francis's sixteen great-great-great-grandparents on the paternal side more than a dozen were adherents of this sect. Stringently selected as the early Quakers were, their rules of intermarriage produced a stern and stubborn breed of highly competent men and women, many of whose descendants are household names today. However, among the early Galtons, John's grandson, Samuel the Second (as he may be called) was almost the only man of note. He moved from Bristol to Birmingham, and married another Quakeress, Lucy Barclay. There he became a member of the Lunar Society, which included Priestley, Watt, Wedgwood, Baskerville the printer, and Erasmus Darwin. Samuel's hobbies were optics and astronomy: University College now possesses a complicated orrery that he once purchased. It was apparently his experiments on colour-mixture which first suggested to his youthful friend, Thomas Young (another Quaker, brought up by the Barclays) those famous researches that led to the three-colour theory.[2]

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[1] For a vivid account of social conditions at that time see Macaulay's *History*, chap. III, ' The State of England in 1685 '. The name Galton is still legible on tombstones in the churchyard at Yatton, a parish in the adjacent county of Somerset, about 12 miles south-west of Bristol.

[2] Thomas Young, *Lectures on Natural Philosophy* (1807). In passing we may note that Young was yet another of those infant prodigies with which Quaker families seem to have abounded—a genius who, strangely enough, finds no place in the Terman–Cox studies. Mathematician, physicist, physician, linguist, antiquarian, scholar, and a Fellow of the Royal Society when only 21, he might serve as a striking instance of an innate intelligence that was truly general.

Nominated by Joseph Priestley, Samuel was the first of the Galtons to be elected a Fellow of the Royal Society. His mechanical skill made him, like Baskerville, a successful maker of guns. But eventually he determined to concentrate talents that might have made him a leading physicist or mathematician on the problems of finance. He died a wealthy banker, worth £300,000. His son, Samuel Tertius, the oldest surviving son in a family of ten, inherited the business.[1] He was elected High Bailiff of Birmingham, and later Deputy-Lieutenant of the county. In 1807 he married Violetta, daughter of Erasmus Darwin, and so became the father of Darwin Galton and of Francis.

On the paternal side, however, the most interesting ingredients were contributed not by the Galtons, but by the Barclays. The Barclays (or Berkeleys, as they were known in Gloucestershire) are one of the three English families which can be traced back to pre-Norman times. Perhaps the most famous was Colonel David Barclay of Ury, a soldier of fortune, who served under Gustavus Adolphus and in our own civil war, " brandishing a sword which other men could scarcely lift ". When he was fifty, so he tells us, he made up his mind to " change to the service of God ", and joined the Society of Friends. American readers will remember Whittier's stirring ballad:

> Up the streets of Aberdeen,
> By the kirk and College Green,
> Rode the Laird of Ury.

Eventually the Quaker Laird married the daughter of another famous Scottish clan, Catherine Gordon. It was their son, Robert Barclay, 'the Apologist', a scholar and a master of English prose, who formulated the traditional doctrines of the Society of Friends. Being himself a direct descendant of the Stuarts, he was able to appeal successfully to James II to check the persecution of his sect. It is, however, surprising to find his sister Jean marrying another rough and reckless warrior, Sir Ewan Cameron of Lochiel surnamed 'the Black'. He was one of the last of the great Scottish chieftains, and in 1689 summoned his clan to join the Jacobite rising to defeat William's lowland regiments at Killiecrankie. Macaulay describes him as 'the Ulysses of the

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[1] Samuel the Third inherited none of Samuel the Second's vigour or versatility. But he possessed the same aptitude for applying quantitative methods to practical problems, and published a small book on currency, with tables. Pearson contends that he passed on his business aptitudes, together with the family's almost obsessive passion for organizing and classifying everything, to his son, Francis. The feminine relatives displayed a similar fondness for method in everyday affairs—filing, labelling, indexing, record-keeping (sometimes with the help of three coloured inks). My father used to say that Darwin Galton managed the Claverdon estate with all the systematic efficiency of a scientist; Darwin Galton himself attributed his innovations rather to the business traditions of a family of Quaker merchants. From Francis's youthful letters one might infer that in his case "the business-like habit of daily book-keeping" was painfully acquired under his father's watchful eye. One of his letters home declares he has now become so well disciplined that "even if I wish to give coppers to a crossing-sweeper, I no longer feel in my trousers; I pull out my pocket book, and examine the balance".

Highlands '.[1] He lived to be ninety. The progeny of this union were men and women of vigorous health and great physical strength, long-lived, and full of the spirit of wild adventure. Their grandson, a Member of Parliament, is reported to have hurled a trespassing donkey over a hedge " as easily as he would a cushion ". His son, Captain Robert Barclay, Francis's great-uncle, was a keen agricultural reformer, and an athlete who " once walked a 1,000 successive miles in a 1,000 successive hours ". When as an old man of 70 he was dining with the Galtons, he lifted Francis's eldest brother (who weighed 12 stone) on the palm of one hand from the floor to the table. It was to this Barclay-Cameron blood that Francis himself ascribed his own " physical toughness and unusual power of enduring bodily fatigue ".[2]

Let us now turn to Francis Galton's ancestry on the maternal side. The earlier Darwins, like the earlier Galtons, comprise few names of note. The pedigree starts about 1500 with four generations of Lincolnshire yeomen. In 1650 one of the younger members of the family became a London lawyer, a member of Lincoln's Inn, and married the daughter of a still more eminent lawyer, Erasmus Earle, ancestor of the novelist Bulwer Lytton, and Own Serjeant to the Commonwealth. Their great-grandson was Erasmus Darwin, M.D., F.R.S., famous in his day as the author of several philosophical poems on biological subjects.[3]

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[1] *History of England*, ch. XIII, which summarizes his remarkable exploits. " Tall and strongly built," says Macaulay, " he was in personal qualities unrivalled among the Celtic princes." The most celebrated of his hand-to-hand fights was the model for the fight between Roderick Dhu (' the Black ') and Fitzjames in Scott's *Lady of the Lake*.

[2] *Memories*, pp. 5, 11. According to contemporary accounts he walked one mile in each hour, and so never slept for more than 70 minutes on end for the whole six weeks. Dr. Barbara Moore, we may recall, completed 871 miles in 526 hours. Many other relevant stories of Galton's Scottish ancestors are related by his elder sister in her *Reminiscences*. She and Francis visited Ury in 1839, and saw the old house with its secret chambers. Their hostess, Margaret Barclay, showed them a miniature of Queen Anne, set with diamonds, presented by the Queen to their Jacobite ancestor, and gave them a hair from Prince Charlie's locks. A branch of the same family later founded Barclay's prosperous bank. One other Quaker family deserves a passing glance—the Freames, who intermarried more than once with the Barclays, and so contributed to the Galton stock. They too exhibit the usual fourfold type of ' social mobility ', beginning as humble farmers, then changing to retail traders, and so to large-scale business, with men of the professional class—clergy, lawyers, and scholars—emerging in the later stages. Their genealogy begins with Robert Freame of Cirencester. His granddaughter married the son of Barclay, the Apologist, and was thus a direct ancestor of Francis. Her father started as a local grocer, and later transferred his business to Lombard Street; her brother, who inherited it, became a Lombard Street banker, and his son-in-law in turn became a partner of Samuel Galton, the Birmingham banker; her cousin emigrated to Philadelphia, and married the daughter of another famous Quaker, William Penn. The Freame pedigree includes Elizabeth Fry, the prison reformer (the Frys were another Quaker family who established the well known chocolate firm at Bristol), William Wordsworth, Christopher Wordsworth (Master of Trinity and brother of the poet), three other Wordsworths who became bishops, Elizabeth Wordsworth (Principal of Lady Margaret Hall), and yet another Fellow of the Royal Society (Sir William Watson).

[3] E.g. *The Loves of Plants* (1789) and *Zoonomia* (1794). As de Candolle observes, these " hit the mood of the moment, but Canning's parody, *The Loves of Triangles*, killed poor Darwin's reputation ". Erasmus's brother, the first Robert Waring Darwin, was the author of a *Principia Botanica* which ran through several editions. There is, however no hint of any scientific bias

Erasmus Darwin married twice; and with each of the two families we encounter stocks of entirely different types from those we have met with hitherto. His first wife was Mary Howard, grand-mother of Charles Darwin. Through her ancestors the Darwin family is linked with some of the most active and famous representatives of the British aristocracy and royalty.[1] Their eldest son was a brilliant medical student, who, before he was twenty-one, was awarded the Aesculapian Society's first gold medal for an experimental research on pus, but unfortunately died from a dissecting wound the following year; the third son, the second Robert Waring Darwin, was, like his father, both a doctor of medicine and a Fellow of the Royal Society. He became the leading physician of Shropshire, accumulated a handsome fortune, and married Susannah Wedgwood, the eldest daughter of Josiah Wedgwood, F.R.S.[2] Their youngest son was Charles Robert Darwin, the celebrated naturalist. He married his cousin, Emma Wedgwood. Three of their sons and one grandson became eminent scientists and Fellows of the Royal Society.

Erasmus Darwin's second wife, the grandmother of Francis Galton, was Elizabeth Collier, widow of Edward Sacheverell Pole (a relative of the famous Dr. Sacheverell who was impeached in 1710). Here we seem to encounter a temperamental strain, differing still more widely from that which characterized the Galtons and the Barclays. This branch begins with two memorable scientists—Sir Henry Savile, tutor to Queen Elizabeth I, and founder of the Savilian professorships of Geometry and Astronomy at Oxford, and Sir William Sedley, founder of the Sedleian Chair in Natural Philosophy. Both were direct ancestors of Francis. Sedley's son John married Savile's only daughter Elizabeth. She was the precocious girl of whom the poet Waller wrote:

> Here lies the learned Savile's heir,
> So early wise and lasting fair
> That none, except her years they told,
> Thought her a child or thought her old.

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among the Darwins before this point; it may have entered the stock through Erasmus's mother Elizabeth Hill, who was very probably a near relative of the botanist, Sir John Hill of Lincoln. As Galton remarked, when the Darwins display a scientific bent, it is biological rather than physical, technological, or mathematical. For the earlier Darwins see *The Pedigree of the Family of Darwin* (1888) by H. F. Burke, Somerset Herald, and E. Krause, *Erasmus Darwin* (1879).

[1] Of the Howards the most celebrated were the Dukes of Norfolk; the first Duke was created Earl Marshal (c. 1440), the second was grandfather of Anne Boleyn and great-grandfather of Queen Elizabeth I. Mary Howard's mother, Penelope Foley, was related directly or indirectly to many other noble families—the Beauchamps, the Devereux, and the Nevilles (each furnishing Earls of Warwick), the De Veres and the De Bohuns.

[2] Josiah Wedgwood, Charles Darwin's maternal grandfather (not mentioned by Pearson) was the famous potter who developed his celebrated ware mainly as a result of his scientific and experimental ingenuity. He was a man of much greater originality than any of the older Darwins. Susannah's brother, Thomas Wedgwood, published in 1802 a paper on 'Copying Paintings on Glass and Making Profiles, by the Agency of Light on Nitrate of Silver', and thus became one of the pioneers of photography: (see Eliza Meteyard, *The Life of Josiah Wedgwood*, 1865 and *A Group of Englishmen*, 1871). I should therefore be tempted to attribute the special characteristics of Charles Darwin's genius as much to the Wedgwood side of the family as to the Darwinian.

Their son was the playwright, Sir Charles Sedley, one of the most profligate of Charles II's courtiers, whose lewdness shocked even Pepys. His lyrics—" Phyllis is my only joy ", " Love still has something of the sea "—adorn almost every anthology. His only child was Catherine, Countess of Dorchester, who succeeded Arabella Churchill as mistress of James II. She was a woman of strong character, sprightly wit, and (if Kneller's portrait can be trusted) not without beauty.[1] She subsequently married David Colyear, a sagacious officer of foot, who served under William of Orange and was raised to the peerage as first Earl of Portmore. Their son, Beau Colyear, the second Earl of Portmore, married Juliana, Dowager Duchess of Leeds. The Duchess engaged a governess for her daughters; and it seems probable that her choice fell on a grand-child of Jeremy Collier, a man of high character and ability.[2] At all events a few years later Beau Colyear had by this governess a natural daughter named Elizabeth Collier, " a lady of great talent and natural charm "; and it was she who eventually became Erasmus Darwin's second wife. It must be, I fancy, to this particular strain in his ancestry that Francis Galton owed his lively humour, vivid imagination, and literary tastes and style, which so sharply distinguished him from the rest of the Galton and Darwin stock, as well as the wayward behaviour which, in his early youth, occasioned not a little anxiety in the mind of his somewhat Puritanical father.

From this brief survey of his pedigree it is plain that Francis was a mixture of many different breeds. The results are perhaps most obvious if we consider his physical build and appearance. Excellent portraits of his ancestors and other relatives still survive—many of them by artists like Reynolds, Raeborn, and Kneller.[3] From the Darwin side he seems to have inherited the brachycephalic skull, with the large and slightly sloping forehead, the flattened occiput, the overhanging brows, and the luxuriant eyebrows that so often go with them—all of which are familiar from the portraits and busts of Charles Darwin, and are equally conspicuous in Erasmus and Robert Waring Darwin, and in Charles Darwin's own sons.[1] However, he did not inherit what the family called the 'Socratic Darwin nose' or the firm and slightly undershot Darwin chin (both of them well marked Wedgwood characteristics),[2] nor yet the heavy, stooping, somewhat clumsy body, which most of the male Darwins tended to develop after middle age. Francis's nose and face, particularly its lower part, were refined and delicate almost to effeminacy—a trait which seems to have entered the stock with Beau Colyear, and appears more or less intermittently in his descendants; it was obvious in the oldest brother Darwin Galton, and still more in his sister Bessie (Mrs. Galton Wheler[3]), both of whom had the same slightly prim expression about the lips that can be seen in later portraits of Francis.[4] Of his other distinctive traits his tall frame, fair hair, and blue eyes were attributed to the Barclay heritage; his strength and endurance to the Scottish elements—the Gordons and the Camerons. Longevity was particularly characteristic of the Barclays, Colliers, and Galtons; many lived to well over 90. The asthma and deafness which afflicted him in later life were typical Galtonian ailments.

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[1] It was originally planned that she should marry the first Sir Winston Churchill; but she herself had loftier aspirations. Macaulay gives a lively account of her career and her fruitless manoeuvres to rescue James from the catastrophe which she foresaw (*History*, ch. VI). According to Macaulay (who quotes Charles II's jests and her own about her looks) her appearance, unlike her intelligence, was decidely homely; but this is hardly borne out by Kneller's portrait, even if we allow for an artist's flattery.

[2] He figures in our history books as the non-juror whose attack on Dryden, Congreve, and Wycherley, in his *Short View of the Profaneness and Immorality of the English Stage*, did much to cleanse the drama of those days. " There is hardly any book of that time," says Macaulay, " from which it would be possible to select specimens of writing so excellent and so varied."

[3] Pearson's plates reproduce over fifty. Most of them at the time he wrote were still at Claverdon in the possession of Mr. Wheler Galton. Several now hang in the National Portrait Gallery. To make a fair comparison one should examine not the plates, but the originals.

The development of post-Mendelian genetics has impressed on us our deplorable ignorance of the way human characteristics are transmitted, and particularly the wide gap that separates hypothetical genotype from observable phenotype. Hence the attempts at tracing the genealogy of a given individual's outstanding peculiarities, which were so popular at the beginning of the century, have long gone out of fashion. Nevertheless, where still accessible, such details seem well worth placing on record, in the hope it may eventually prove worth while taking up such speculations once again. In what follows I have tried to note more especially those features and characteristics of body build which Mendelian writers themselves have often held to be ' inherited as unit characters' (if I may borrow a succinct but somewhat misleading phrase).

It is tempting to speculate in much the same fashion about the sources of his intellectual and temperamental qualities. But, unlike so many bodily traits, mental tendencies show little or no discernible indication of unifactorial transmission. And in the Darwin-Galton pedigree almost the only mental characteristic we can confidently assign to hereditary endowment is the intellectual precocity and high intelligence which Francis showed even more conspicuously than

[1] Francis was examined by phrenologists on two or three occasions. While he was at King Edward's School one of the Cambridge examiners, an enthusiastic follower of Combe, asked to inspect the heads of the scholars in order that he might compare the results with the marks gained in the examination. So impressed was he by Francis's skull that he sent for him a second time. "This boy," he told the headmaster, "has the largest organ of causality I have ever seen, except in the case of Dr. Erasmus Darwin." "Why," replied Dr. Jeune, "he is Dr. Darwin's grandson."

[2] Charles Darwin's face (though not his skull), like those of several of his sisters and sons, shows a striking resemblance to the face of his mother, Susannah Wedgwood. I may add that, in common with other families of those days, both the Darwins and Galtons fondly discussed points of family resemblance or difference in their children, and sought to account for them by referring to their various ancestors. Francis himself took a keen interest in head shape and physiognomy, and later endeavoured to study them by more exact methods.

[3] Her face, in the portrait which depicts her at the age of 80 and formerly hung in Claverdon, might almost be mistaken for that of Francis. Seen side by side, as I saw them in later life, Francis and Darwin Galton were unmistakably brothers. Yet in appearance all three were quite unlike their father or mother. On the other hand, there is a striking resemblance between Francis and his uncle Sir Francis Sacheverel Darwin (grandson of Beau Colyear); and both were considered by family to resemble Catherine Sedley (the Countess of Dorchester) and Sedley the poet.

[4] Perhaps the best account of Galton's physical appearance in later life is that given by Mrs. Sydney Webb (later Lady Passfield). Among the many eminent scientists with whom she became acquainted, she says, "the one who stays in my mind as the ideal man of science is, not Huxley or Tyndall, Hooker or Lubbock, still less my guide, philosopher, and friend Herbert Spencer, but Francis Galton, whom I used to observe and listen to with rapt attention. Even today I can call up that tall figure, with its attitude of perfect physical and mental poise, the clean shaven face, the thin compressed mouth with its enigmatic smile, the long upper lip, prim chin, and, presiding over the whole personality, the prominent dark eyebrows, from beneath which gleamed, with penetrating humour, contemplative grey eyes" (*My Apprenticeship*, 1926, pp. 134–35).

the other members in this family tree. Terman assesses his I.Q. as approximately 200, and that of Charles Darwin, with less assurance, as between 135 and 140.[1] An exceptional measure of intelligence is traceable in one or more members of every one of the ten generations for whom we have first hand information—right back to the Sedleys and the Saviles of the 16th century. The steady ascent in social rank of the Galtons, the Barclays, and the Darwins, manifestly implies a degree of ability that must have been well above the general average, and suggests a cumulative process of social selection. The later generations include eleven Fellows of the Royal Society—surely a record. Francis himself was elected in 1856, and later was awarded the Gold Medal, the Society's Copley Medal, and in 1902 its Darwin Medal. Both his grandfathers[2] were Fellows, two of his cousins,[3] one of his uncles,[4] three of his nephews, and one great-nephew.[5]

To assign the nature and origin of his more specialized abilities would be a far more precarious venture. He was, like the Darwins, a man of science rather than a man of business or a man of letters; and, like his famous cousin, Charles, he displayed in an extraordinary degree what McDougall once described as " the exploratory and inquisitive disposition which characterizes so many of the world's inventors and discoverers ".[6] Yet, unlike his cousin, he appears also to have been endowed with a touch of the poetic imagination of the Sedleys, and a good share of their literary skill and humour, and (with several other members of the Galton family) a strong mechanical bent.[7] In the main, however, his mind (to use the familiar antithesis) was analytic rather than synthetic—highly

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[1] As Sir Gavin de Beer has pointed out, " Darwin was a late developer —a peculiarity that was clearly a function of his genetic make-up because the same feature was characteristic of his brilliant children " (' Charles Darwin: A Master Mind ', *Proc. Brit. Acad.*, XLIV, 1958, p. 181). Throughout his later life indeed " the mills of his mind, like the mills of God, ground slowly "—and this was just as well, considering the mass of sifted evidence on which his great theory depended.

[2] Samuel Galton the Second, and Erasmus Darwin.

[3] Sir Douglas Galton, K.C.B., and Charles Darwin.

[4] Robert Waring Darwin.

[5] Sir George, Sir Francis, and Sir Horace Darwin, Sir Charles Galton Darwin, son of Sir Horace (Professor of Natural Philosophy at Edinburgh and later Director of the National Physical Laboratory). The eleventh Fellow in my list is Josiah Wedgwood, Charles Darwin's maternal grandfather, who was not directly related to Galton.

[6] Dr. Eliot Slater describes both Darwin and Galton as showing a " compulsive curiosity " and the Galton family as exhibiting an " obsessional desire to count or classify " (' Galton's Heritage ', *Eugen. Rev.*, LII, 1960, pp. 91f.). Psychiatrists are prone to express the exceptional manifestation of any common characteristic by a pathological epithet. If we discount the psychoneurotic implications, we may, I think, accept both descriptions. Darwin in one of his letters relates how he has been speculating about the characteristics which make for originality or creativity—or, in his own words, " what makes a man a discoverer of undiscovered things ": it consists, he thinks, " in habitually searching for the causes and meaning of everything " and " implies (amongst other things) sharp observations ".

[7] Darwin, it may be recalled, declared that during his life he had been " singularly incapable of learning any language "—a marked contrast to Galton. " Versification," he adds, " I could never do well." On the other hand, even before he was eight, his " taste for natural history, especially for collecting, was very strong and clearly innate " (*Autobiography*, pp. 2f.)

Galton's literary skill is seen best in his youthful writings; but even in his old age his sense of style is revealed by his comments on contemporary scientific writing. The Cambridge enthusiasts curious, highly critical, tending not merely to observe details, but to classify and organize the details into a rational scheme; and with this went the peculiar fondness for counting and calculating which (as we have noted) seems to have been an almost obsessive propensity in several of the Galtons. He certainly inherited much of their remarkable energy and persistence. Yet in temperament he differed widely from his parents and from most of his surviving brothers and sisters. He was, as the family frankly observed, " decidedly restless "—displaying even up to middle age the *Wanderlust* and love of change that had been characteristic of so many of his Scottish ancestors. It is perhaps to this constitutional restlessness that he owes his amazing versatility. The industry, the extreme conscientiousness, and the businesslike habits—the distinctive characteristics of his father and indeed of so many eminent Quakers—were, I fancy, rather painfully acquired during his upbringing, and not, as Pearson supposes, literally inherited.

II. CHILDHOOD AND EDUCATION

Francis was the little Benjamin of his family—the last of nine children. Of the others the youngest boy was already seven and the youngest girl already eleven when he was born, so that within the family he had no playmates of his own age. " He was," says Mrs. Galton Wheler (the oldest of the lot), " the pet of us all "—a circumstance that may account for several traits that he exhibited as he grew older. His sister Adèle, who, owing to a curvature of the spine, was something of an invalid, devoted her time to his early studies. " She made him learn his letters in play, and he was able to point to each before he could speak " (I am again quoting Mrs. Galton Wheler); " and she taught herself Latin and Greek so that she might teach him." By the time he was 2½ he could read simple fairy tales; and he could sign his name before he was 3. The following is the first of many letters treasured by the family.

" My dear Adèle, I am four years old, and I can read any English book. I can say all the Latin substantives, adjectives and active verbs, besides fifty-two

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who today are so eager to " bridge the two cultures " might well refer to Galton's paper on ' Suggestions for Improving the Literary Style of Scientific Memoirs ' (*Trans. Roy. Soc. Lit.*, XXVIII, pp. 1–8). Here he criticizes in some detail " the comparative rarity among the English of a sense of the difference between good style and bad, especially noticeable among younger scientific men whose education has had little concern with the Humanities ".

Dr. Slater very justly notes the fact that, despite his ' feminine streak ' and ' acute sensibilities ', Galton seems to have been almost wholly devoid of aesthetic interests: " art, whether in colour or form, was no essential need for him "; he was " totally unmusical ", and during his travels quite " unimpressed by the magnificent scenery ". I fancy Galton himself has provided the clue. " Nearly every Quaker is descended from men and women who dressed in drabs; and one of their strongest opinions was that the fine arts were worldly snares: a born artist could never consent to separate himself from his fellows on such grounds ", and would therefore have " deserted ". This process, he thinks, may have been responsible for the Quaker's lack of " the temperament associated with a love for colour ", and incidentally for the well-established fact that colour-blindness is " nearly twice as prevalent among Quakers as among the rest of the community ". Dalton, who discovered colour-blindness as a personal peculiarity of his own, was " a Quaker to his death "; Young, who became greatly interested in the study of colour, ceased to belong to the sect (*Human Faculty*, pp. 32f.).

lines of Latin poetry. I can cast up any sum in addition and multiply by 2, 3, 4, 5, 6, 7, 8, (9), 10, (11). I read French a little and I know the clock. Francis Galton. Feb(r)uary 15, 1827."[1]

When he was five, he went to a kindergarten school; and one afternoon the maid who went to fetch him discovered him holding a handful of youngsters at bay, and shouting

> " Come one, come all! This rock shall fly
> From its firm base as soon as I."

An aunt who visited the family when he was six describes him as " a little prodigy, reading Pope and Shakespeare for pleasure. He has only to go over a page a couple of times and he can repeat it by heart." The next year he was studying Greek; and, when an academic friend kept quizzing him about Homer's *Odyssey*, Francis at length replied, " Pray, Mr. Horner[2], look at the last line in the twelfth book." The last line (as Lang translates it) reads: "All this I have told thee already, and it likes me not to repeat a twice-told tale."

On his eighth birthday, when he was sent off to a school at Boulogne, he drew up a semi-humorous ' Last Will and Testament ' bequeathing " to my dearest sister Adèle my Watch, my English books, and Collection of Beetles; . . . to Bessy my Minerals and Shells; to Lucy my Hygrometer; to Darwin my parchment; to Erasmus my Bow, Arrows, and Steel Pens . . . And I make my dearest sister Adèle my executrix. Signed, sealed and delivered " . . . etc. The items quoted display his early interests. In his new school he was placed in one of the highest classes, with boys ranging in age up to 15. Both his father and his various masters recognized that he was a lad of great ability; but none of them recognized *how* great it was. From the last of the two later schools to which he was sent, King Edward's School, Birmingham, he wrote several letters to his father and sister, begging them to arrange for him to do work of a higher and broader type, following up the algebra and geometry, the dynamics and the chemistry, the history and the English poetry that he had studied at home. " I am not getting on in the least," he complains, " Every day is a day wasted . . . Only six books of Euclid is not much for two years." He expressed the same regrets when he came to write his *Memories*. " The time spent there," he writes, " was a period of stagnation: I learnt nothing, and chafed at the limitations. I craved for an abundance of good reading, solid science, and well taught mathematics."[3]

[1] The ' 9 ' has been erased and the ' 11 ' has a slip of paper gummed over it; it is not clear who corrected the spelling of the month.

[2] The visitor was Leonard Horner, the first Secretary (or rather ' Warden ' as he preferred to be called) of University College. According to the College records his professorial colleagues reacted to his " persistent intermeddling " much in the same way as little Francis, though not so speedily or effectively.

[3] Darwin said much the same of his time at " Dr. Butler's great school at Shrewsbury ", a school now so proud of his name. " Nothing could have been worse for the development of my mind, for it was strictly classical: the school as a means of education was to me simply a blank." The accounts of both of them remind one of Osbert Sitwell's entry in *Who's Who*, under the heading of ' Education ': " During the holidays from Eton."

His mother earnestly desired that, since the family business had been wound up, Francis, like her famous father and her brothers, should become a doctor. Accordingly, an uncle by marriage (Dr. Booth) agreed to sponsor his entrance as a House Pupil at the General Hospital, Birmingham, "at the rate of 200 guineas per annum". And so, after spending a week in London with Charles Darwin for Queen Victoria's coronation, and then making a holiday trip on the Continent with another young medico, he started at the age of sixteen to "go the round of the wards with the surgeons and physicians". "I begin to understand all the humbug of medicine, which is not a little!", he writes a few weeks later. "Doctors have the fault of parsons—of being much too positive . . . Cut a brace of fingers off yesterday, and one the day before. Happy to operate on any one at home!"

The following year, however, he was again in London attending medical lectures at King's College. There he found the laboratory work, and particularly the 'legal logic' of forensic medicine, far more congenial than dispensing pills or drawing out teeth. "It had" (he says in his *Memories*) "a sort of Sherlock Holmes fascination." At the examination he took second place, and was "much vexed at not being first". But, he adds, now he has "spoken to Charles Darwin about Cambridge, who recommends going there next October, and to read mathematics like a house on fire . . . They (mathematics) are exceedingly interwoven with chemical and medical phenomena." In the meantime he asks permission to spend the summer vacation with another student "at Giessen, the laboratory of Liebig the first chemist in the world." Once there, however, he was seized ("like a migratory bird", as he afterwards said) with a passion for travel; and he writes to his father that he has resolved "to make a bolt down the Danube and see Athens and Constantinople"—a courageous decision in those days for a lad of only eighteen.[1]

In October 1840 Francis and his father arrived on the top of a stage coach before the famous gateway of Trinity College, Cambridge. Cambridge in those days could boast neither a Natural nor a Moral Science Tripos. Since he had chosen Mathematics, his tutor set him, "with a sort of grin", to study Conic Sections. The daily time-table he found pretty strenuous. "Up for Chapel at 7," he says in the first letter written after he had settled down; "Lectures 2 hours a day; Reading (full tide) $10\frac{1}{2}$ hours—really too much." To stimulate the flagging brain, he explains, he had invented a 'Gumption-Reviver'; and a sketch of the apparatus is appended. It depicts a kind of gallows supporting a funnel of water that drips on to a napkin enveloping the student's drooping head; the flow has to increase with time, for, "as the hours of study increase in arithmetic ratio, so the weariness increases in geometric ratio: unfortunately some of the water has splashed on to my notepaper."[1]

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[1] "From the age of nine", he says, "I had saturated myself with Byron's poetry, which gave me a longing to see the East." His original plan had been to pass the summer shooting reindeer in Lapland; the itinerary he worked out would have followed a route known only to the reindeer hunters themselves. This was very wisely turned down by his parents. Much of the journey down the Danube was made in a small rowboat, accompanied only by his English companion, an aged Austrian, and a boy, toiling at the oars by night and day.

Of the surviving manuscripts relating to this period, one, dated 1841, includes a long poem, headed with the motto *Tu Marcellus Eris*, which celebrates the birth of Edward, the future Edward VII, and traces the baby's descent from Edward II, the first prince of Wales:

How different is thy lot to Edward's son
Born in the land his sire had scarcely won,
'Midst warriors rude within that turret tall
That beetles o'er Carnarvon's massive wall.

It was presumably composed for the Chancellor's Medal, obtained in that year by one of Galton's closest friends, H. S. Maine.[2] In the end, however, Galton found the work for an Honours degree in Mathematics at once too exacting and too dull. His health suffered, and so, like Charles Darwin, he eventually contented himself with a Poll degree.[3]

III. TRAVEL AND EXPLORATION

Early Scientific Work. Galton's adult life may be divided into three main periods; during the first (1844–1864) he was chiefly occupied with travel and exploration, and with various geographical and meteorological studies arising out of his adventures; during the second (1865–1899) he became more and more engrossed in the theoretical study of individual differences, with the problems of inheritance as the focus of almost all his researches; during the third (1900–1911) his interest changed to the more practical aspects of the subject, and he came forward as the champion of eugenics.

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[1] His letters to his father are continually illustrated by ingenious mechanical designs, some imaginary, some actually constructed—for instance, a 'patent lamp', and an 'unpickable lock'. The latter he demonstrated to the representative of a famous firm of safe-makers; when his visitor extolled an elaborate lock made by his firm, Galton at once showed how it could be picked with the help of a set of knitting needles of various sizes—a procedure which (so he facetiously declared) he meant to patent "For the Use of Thieves, House-breakers, and Others". The earliest of his many mechanical devices, dated July 1835 (when he was only 13), is entitled "Francis Galton's Aerostatic Project": it was apparently a flying machine with flapping wings to be worked by an oscillating steam-engine. One of his most ambitious inventions was a printing telegraph, designed ten years later (about 1845) as a result of a number of extensive experiments—the first instrument of its kind so far as I am aware (*The Teletype*: *A Printing Electric Telegraph*, by Francis Galton, M.A., John Weale, 1859, pp. 32). Whether the complete instrument was ever manufactured is unknown; but it appears to have interested one or two of the commercial companies that had recently introduced telegraphy into this country. The Eugenics Laboratory contains numerous bits of mechanism and mechanical contrivances, the purpose of which has long been forgotten.

[2] One of the most gifted scholars produced by Christ's Hospital, who afterward, as Sir Henry Sumner Maine, gained great celebrity as a student of primitive custom and law.

[3] In the examination, so he tells his father, "classics was below par; as for medicine, etc., I was only allowed a month to get my subjects up; in Mathematics I was third, and would have been first but for misunderstanding one question". (This period of Galton's life, and particularly the influence of his medical studies on his later psychological investigations, has been more fully discussed in Dr. Charlotte Banks' paper, presented at the last meeting of the British Association and appearing in a forthcoming issue of the *British Journal of Educational Psychology*.)

In October, 1844, the year in which Francis left Cambridge, his father, Tertius Galton, died. Released from the rigorous Quaker supervision, which hitherto had regulated almost every step, he seems to have experienced a sharp reaction. " I was free," he wrote later on, " and eagerly desired a complete change. Besides I had many wild oats to sow . . . So, finding I had a competent fortune, and disheartened by the sense that medical knowledge was so lax and progress apparently barred, I determined to give up a medical career. My passion was for movement and travel." The following year he set out quite alone for Egypt and the Sudan. There one of the Beys said to him: " Why follow the usual routine of going only to the Second Cataract? Cross the desert and see Khartoum." And so he did. For a while (he tells us) he " lived a very oriental life ", dressed in Arab costume, and became " fairly fluent in Arabic ". After a succession of adventures and the death of his devoted servant Ali, he at length worked his way home via Jerusalem and the Holy Land, " escorted by mounted spearmen, and bringing back a couple of monkeys ".

On his return to Claverdon, whither his mother and sister had retired, he appears at first to have thought of settling down, like his elder brother, Darwin Galton, to the life of a sporting country gentleman.[1] On his brother's advice he joined the Leamington ' Hunt-Club ', and the next four or five years were passed in hunting and shooting with a set of wealthy young men from Warwickshire, noted for their recklessness and extravagance. The attractions of gambling, which ruined a good many of them, he managed to resist by " viewing it in the light of mathematical reasoning." His most exciting experience appears to have been an ascent by night in a balloon from Cremorne Gardens with an aeronaut and a boy, landing miles away on the lawn of an irate squire.

Galton's next expedition, to tropical South Africa, had a more serious purpose. In those days the exploration of ' darkest Africa ' had much of the fascination that the exploration of ' outer space ' possesses today. Soon after the independence of the Transvaal had been formally recognized, the neighbouring territory of Bechuanaland, which had been opened up by missionaries from the Cape like Moffat and Livingstone, began to assume international importance. But of the territory on the opposite side of Bechuanaland, between the Kalahari Desert and the West Coast, practically nothing was known. This Galton determined to explore, starting from Walfish Bay. As his travelling companion he engaged a young Swede named Anderson, who had been brought up by an English Quaker.

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[1] See *Memories*, ch. VIII (' Hunting and Shooting '). In Victorian Warwickshire, as I can testify from my own recollections, there was a sharp distinction between social classes; the ' gentry ', as they were called (i.e. members of the older county families, like the Dugdales, or of the ancient aristocracy, like the Warwicks and their circle) resented any intrusion from those connected with ' trade ' (such as the rich industrialists from Birmingham). However, in the second or third generation the younger members, still a little ashamed of their commercial origins, often succeeded, usually by going first to one of the older universities and then dropping into the role of landed proprietors, in winning a tardy acceptance. The Galtons were typical instances of this particular form of ' social mobility '. Later on it became one of Francis Galton's most cherished aims to substitute a genuine ' meritocracy ' for the ' relatively decadent aristocracy of birth '.

They found the country sparsely populated with four very different tribes—the Namaquas, 'a superior Hottentot race living on the edge', the pastoral but warlike Bantus in Damaraland, small yellow Bushmen in less fertile parts of the lowlands, and a fugitive Negro race on the hills—each of whom provided Galton with numerous problems in comparative anthropology.[1] Owing to the inhospitable nature of the region, they ran short of supplies. Then lions killed a mule and a horse: the carcases they recovered and used as food. Galton succeeded in making friends with the " murderous Damaras " by investing a chieftain with a theatrical crown " bought in Drury Lane for some such purpose "; and eventually the party worked their way for more than 1,000 miles inland " through a country never before penetrated by a civilized being ".[2] On their return a detailed account of the journey and a valuable survey of the area was published, with maps and woodcuts based on Galton's sketches.[3]

The following year, 1853, Galton married Louisa Butler, daughter of the Dean of Peterborough and former headmaster of Harrow, sister of a still more famous headmaster of Harrow, who subsequently became Master of Trinity College, Cambridge. After his marriage Galton's travels virtually ceased: otherwise it might have been Galton, and not Stanley, who set out to discover Livingstone. In 1856, however, as Secretary of the Geographical Society, he drafted the instructions for the Burton-Speke Expedition from the East Coast, which led to the discovery of Lake Tanganyika and Lake Victoria Nyanza.[4] Meanwhile, he himself set about compiling 'Hints to Travellers' and charts of the local climate to aid later explorers.[5] These were followed by meteorological charts for the British Isles—the first weather maps ever to be published. And on April 1, 1875, the readers of *The Times* began to see at their breakfast tables diagrammatic maps with isobars and the like, very much in the form with which we are familiar today. It is to Galton and to his meteorological studies that we owe the name and the whole notion of the anticyclone. Had he done nothing else, he would still have deserved fame as an early pioneer in the science of meteorology.[1]

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[1] See *Human Faculty*, pp. 200f., and various papers in the *Journal of the Anthropological Institute*.

[2] *The Times*, quoting the address of the President of the Society (Sir Robert Murchison) on Galton's return: we are told that the " astronomical observations determining latitudes and longitudes were made most accurately by Mr. Galton himself ".

[3] *The Narrative of an Explorer in Tropical South Africa* (1853, 2nd ed. 1889). Galton was awarded the gold medal of the Royal Geographical Society, and in the following year the silver medal of the French Geographical Society. After 1884, when it was annexed by Bismarck, till 1920, the region was known as German South-West Africa.

[4] According to Ptolemy, a Greek merchant named Diogenes returning from India, somewhere about the middle of the first century, landed on the African coast near Zanzibar, and claimed " after passing snow-capped mountains" (the so-called 'Mountains of the Moon', i.e., Mount Kenya and Mount Kilimanjaro) " to have reached the vicinity of two great lakes from which the Nile draws its twin sources ". In the early 1850's Arab slave-traders reported similar stories related by the natives. The Royal Geographical Society was sceptical: how could snow exist so near the Equator? To answer these questions a special expedition was planned. As the man who at that date knew as much as anyone about the problems of African travel, Galton was strongly urged to undertake it. But he was still suffering from a malarial infection caught during his earlier travels. In Kensington Gardens there is a solitary red obelisk dedicated to the memory of Speke. Galton later urged that the memorial should be extended to commemorate still more famous names—Burton, Livingstone, Stanley, Baker, and Grant. Surely Galton's own name might with justice be included.

[5] These were followed in 1855 by *The Art of Travel*, a guide for explorers. Here incidentally he relates an occurrence which admirably illustrates what later became his ruling maxim—' Whenever you can, measure or count '. In Damaraland he met a typical Hottentot Venus whose buttocks were so immense that he felt they should be accurately measured. Yet any such approach was found

IV. Psychological Studies

Galton's observations made during his travels began more and more to turn his thoughts from man's environment to man himself. " Before the appearance of Darwin's *Origin of Species* in 1859," he tells us, " I had already begun to interest myself in the human side of geography. Some ideas I had about human heredity were set fermenting; and I then began a book on *Hereditary Genius*." The leading ideas were embodied in a preliminary paper entitled " Hereditary Talent and Character ", published in *Macmillan's Magazine* in 1865. The wording of the title is significant. " The terms talent and character," he says, " are exhaustive; they are meant to include the whole of man's spiritual nature." In writing *Hereditary Genius*,[2] however, he appears to have preferred a tripartite to a dual classification of mental characteristics. He now speaks of three main ingredients—the ability to work, an interest in the work, and the power or will to work, corresponding to what Ward and Stout would have called the cognitive, the affective, and the conative aspects of mental activity. It is principally with the first—with ability or talent in its various forms—that his book is concerned.

And here he introduces at the very outset a new and far-reaching distinction —the distinction between *general* ability and *special* abilities. The current

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to be misinterpreted. He resolved his dilemma by the device employed by Gulliver's Laputan tailor: " with the aid of navigational instruments " he measured the lady's dimensions " from a distance ".

[1] See his papers on ' Meteorological Charts ' (*Phil. Mag.*, 1861) and on ' A Development of the Theory of Cyclones ' (*Proc. Roy. Soc.*, XII, 1862, pp. 385f.). Since the general theory as he developed it envisaged vertical movements of the air as well as horizontal, he pressed strongly for studies of the upper atmosphere by means of unmanned balloons, the firing of guns and rockets, and other experimental devices which have become a commonplace today. From 1860 or earlier until 1901 (when deafness forced him to retire), Galton, in addition to his other work, was actively engaged in meteorological studies as a member of the new Meteorological Committee, and as a chairman of the Kew Observatory. Several papers written about this time deal with unexpectedly topical problems—e.g., a plan for a decimal coinage (10 mites=1 cent; 10 cents=1 florin; 10 florins=1 pound), and a scheme for interstellar communication, by broadcasting mathematical formulae (such ratios as π, $\sqrt{2}$, etc.), and so building up a code for concrete and abstract ideas. See more particularly *Memories*, chap. XVI.

[2] 1869. A cheap edition (1962, 8*s.* 6*d.*) with an introduction by Professor Darlington is now available in Collins' Fontana Library.

faculty psychology recognized only the latter.[1] Galton insists on the superior importance of the former, a doctrine borrowed later on by both Binet and Spearman. " Numerous instances recorded in this book," he says, " show in how small a degree eminence can be considered as due to purely special powers. People lay too much stress on apparent specialities, thinking that because a man is devoted to some particular pursuit he would not have succeeded in anything else. They might as well say that, because a youth has fallen in love with a brunette, he could not possibly have fallen in love with a blonde. As likely as not the affair was mainly or wholly due to a general amorousness." It is just the same, he argues, with mental exploits. And accordingly what he chiefly proposes to discuss is the inheritance of general ability.

Other writers in the past had, of course, considered the possibility that genius might be inherited, some defending the notion, others attacking it. Galton himself, however, claims to be " the first to treat the subject in a statistical manner and to arrive at exact numerical results ". In the preliminary discussion he begins by suggesting that people can be graded for ability in accordance with the normal distribution; and here, for the first time, we find this famous curve applied to mental differences. He proposes to divide the entire range into eighteen classes, and makes the subdivisions between each approximately equal to the so-called ' probable error '. The term ' genius ' is defined as covering the top three classes, that is to say, the brightest 248 in a million: thus very roughly a genius is the ablest person in a random sample of 4,000. He points out that, since the distribution is symmetrical, the same principles could be used to define imbeciles and idiots; and for these he uses a method of assessment which anticipates the ' mental ratio ' or I.Q. A person in the lowest grade of deficiency, he says, is roughly capable of working with the efficiency of one-third of an average man; a person in the next higher grade of working like two-thirds of a man.

To demonstrate his theory of inheritance Galton examines the pedigrees of nearly a thousand geniuses—judges, generals, statesmen, scientists, poets, painters, and divines. Among their relatives he discovers 89 equally eminent fathers, 114 eminent brothers, and 129 eminent sons. If for simplicity we suppose that the number of male children born to each member of the population, whether a genius or not, is on an average four, then the chances that the son of a genius will himself be a genius would be 129 times as great as that of a parent chosen at random; and, since four is obviously much too high a figure, our estimate of the chances must be much too low. Galton, of course, was well aware that the environmental advantages which an able father could provide for his children might well be partly responsible; and both in this book and in later papers he deals at some length, though not perhaps convincingly, with this complication.

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[1] The doctrine itself, however, is at least as old as Aristotle; and it might certainly be read into St. Paul's adaptation of Menenius Agrippa's fable of the body and its members (*Livy*, II, 32) and his statement that " there are varieties of gifts, but one pneuma " (i *Cor.* XII, 4; this chapter is perhaps more frequently quoted in Quaker literature than any other from St. Paul).

The ideas and methods outlined in *Hereditary Genius* were applied to a more specialized problem in his book on *English Men of Science: Their Nature and Nurture* (1874). Today in view of the current demand for scientists some of his conclusions are still well worth studying. From his inquiries he calculated that there were in the British Isles about 300 males between the ages of 50 and 65 who had achieved recognition as men of the highest scientific ability, but that this was well below the number we might expect on the assumption of a normal distribution of ability. Now there exists, so he argues, an immense deal of national work which none but men of scientific culture are qualified to undertake. "What qualities then ought we to look for in those who should be encouraged to take up science?" Some light, he believes, may be gained in analysing the qualities possessed by those who have actually achieved recognition. On the basis of questionaires addressed to 180 men of science, chiefly Fellows of the Royal Society, he enumerates the following special characteristics, in addition to a high degree of general ability: (1) Physical and Mental Energy, (2) Health, (3) Perseverance, (4) Business Habits, (5) Memory, (6) Independence of Character, (7) Mechanical Aptitude. These, it will be noted, are all characteristics which Galton himself exhibited in a signal degree.[1]

Of all his books, however, the one of greatest interest to the student of psychology is that published in 1883 under the title *Inquiries into Human Faculty and its Development*. Here Galton begins by emphasizing the wide extent of individual differences. "The moral and intellectual wealth of a nation," he maintains, "consists largely in the multifarious variety of the gifts of the men who compose it": the endeavour to level them all to an identical pattern would be "the reverse of an improvement" and in any case would be doomed to fail. After a brief discussion of bodily measurements and of the influence of hereditary and environmental conditions on bodily height and health, he proceeds to summarize the main results of the relevant experimental and statistical investigations which he himself had already carried out on individual differences in various mental and moral 'faculties'.

We then reach what is really the central problem of the whole book—the analysis of individual personality as the joint effect of two distinct but complementary factors, 'Nature' and 'Nurture'.[2] "Man," says Galton, "is so educable an animal that it is difficult to distinguish between that which has been acquired through education and circumstance, and that which formed the original grain of his constitution. Different aspects of his multifarious character respond to different calls from without . . . The same nation may be seized by military fervour at one period, and by a commercial at another; the love of art or of science, of religion or of gaiety and adventure, may be paramount at different times." In short, " the interaction between nature and circumstance is so close that it is difficult to distinguish them with precision ".

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[1] There is now a good deal of evidence to show that the assumption of a strictly normal distribution leads to an underestimation of the numbers showing marked deviations in either direction. Ability, like stature, is almost certainly influenced by relatively rare 'major genes', which are responsible for large effects (usually, but not always, in the downward direction) as well as by so-called 'polygenes', which produce small but cumulative effects, and obey the familiar laws of multifactorial inheritance.

[2] The phrase is not, as a recent critic supposes, "an alliterative antithesis of Galton's coining", but borrowed from Shakespeare (*Tempest*, IV, i, 188–9).

The safest mode of approach, he believes, is to start by examining the more important ways in which post-natal experience operates, in other words, the effects of ' Nurture '. This he treats under two main headings as consisting of *ideas* and of the *associations* that introduce or suggest them. Ideas, he maintains, are conveyed by means of mental ' images '. At first sight this seems scarcely possible in the case of abstract ideas. But Galton argues that an " *abstract* idea is essentially a *cumulative* idea "—i.e. a kind of ' generic image ', analogous to the ' generic portraits ' that he had obtained by means of ' composite photography '. He is, however, far more interested in the different types of ' concrete images '.[1] These he had investigated in considerable detail by means of his well known questionaire; and a large part of the book consists of a systematic survey of the unexpected results so obtained. As he rightly points out, the whole inquiry reveals, perhaps more clearly than any other, startling differences between the mental processes of different individuals. And he goes on to indicate the practical values of such studies both for educational and for vocational guidance.

In these and many other investigations he repeatedly stresses the value of controlled introspection, and adduces a number of instances to show that by its means we can discover numerous illuminating facts which would never have been suspected had we restricted ourselves to the mere observation of outward behaviour. The information gained in this way, he says, often accounts for conduct that would otherwise be wholly inexplicable: for example, he notes with astonishment how completely devoid of visual imagery many highly educated persons prove to be, particularly scientists. On the other hand, perfectly normal people whom he encountered often experienced images of such hallucinatory vividness that they mistook them for actual perceptions. And almost invariably the two types of person—the extreme visualizers and the extreme non-visualizers—took it for granted that their own type of mentality was universal. Introspection alone reveals how widely, in cases such as these, the mental processes of different individuals differ from one another.[2]

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[1] ' Generic images ' are more fully described in an appendix, which is abridged from a paper published four years previously. In the text the discussion of concrete imagery, number forms, colour associations, etc., is to be found (somewhat out of place, I fancy) just before the general discussion of nature and nurture. Fechner had already published introspective reports on the visual imagery of several of his subjects, and had briefly analysed their distinctive characteristics, mainly in relation to after-images (*Elements der Psychophysik*, 1860, II, pp. 469f.). Galton's research is noteworthy because of his use (i) of a standardized questionaire and (ii) of a rating-scale (based on an ' ogival ', i.e. cumulative normal, curve) to assess differences for which no direct mode of measurement is available—devices which were rapidly taken up by later psychologists.

[2] Part of Galton's research included an inquiry which in some respects anticipated the ' Census of Hallucinations ' carried out a little later by the Society for Psychical Research. The problem was apparently suggested by a couple of personal experiences related to him in confidence by Sir Risdon Bennett, F.R.S. (the eminently sane and matter-of-fact President of the Royal College of

However, in thus attempting to discover by means of inner observation how our own minds work, we are baulked, he says, by a special difficulty: "It would seem almost impossible to give the required attention to the processes of thinking, and yet go on thinking as freely as if the mind were in no way preoccupied." Nevertheless he believes that in some of his experiments he "succeeded in evading this difficulty". Thus in his study of 'associations' he employs an ingenious procedure (what textbooks now call 'the method of free association') which "enables us to drag into light thoughts, ideas, and wishes that have lapsed out of ordinary consciousness", and so helps us to unravel the associative processes by which they have been acquired. Once again he gives detailed results from experiments upon himself and others, which he analyses statistically.[1] He makes a systematic comparison of the dates at which the associations so disclosed were apparently formed. Of the associations that occur most frequently (that is, at least three or four times), the majority, he finds, are drawn from childhood, and comparatively few from recent experiences. To interpret his results he suggests that we may think of the mind as divided into two or three fairly distinct compartments according to the degree to which the activities taking place are "lit up by consciousness". "There seems to be," he writes, "a presence-chamber where full consciousness holds court, and an antechamber just outside, crowded with ideas lying beyond the ken of

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Physicians), who on two occasions saw a man in mediaeval costume enter his study and then vanish. Galton notes the various ways in which such 'visions' are usually distinguished from exceptionally realistic 'visualizations' (e.g. by their sudden, involuntary, or unexpected appearance, their abrupt disappearance, and 'an indescribable difference in quality'). He was surprised to find how frequent they were, not only in the sane, but more particularly in the eminent or gifted. The 'faculty', he believes, is commoner in childhood, but owing to repression tends to atrophy. "Let the tide of opinion change and grow more favourable to supernaturalism, and seers of visions will come once again to the front." In view of the recent renewal of interest in "spontaneous paranormal phenomena" the whole inquiry is well worth re-studying and perhaps repeating. Another type of 'vision', which many of his subjects (including Karl Pearson) described, consisted of what he called 'phantasmagoria'—shifting patterns, changing faces, scenic panoramas, filmy sheets of printed or tabular matter, quite vivid and yet unreadable—seen perhaps as hypnagogic imagery, but also quite often during the day usually when the eyes are closed. This too deserves renewed attention, particularly in relation to other indications of spontaneous cerebral activity.

[1] One of his incidental discoveries was the possibility of what was later termed 'imageless thought'. The problem of 'abstract' (or, as Galton preferred to say, 'generic') ideas had long been the subject of discussion among English philosophers. At the time Galton started his experiments the accepted view had been stated very succinctly by Burke, who contended that "when a word is unaccompanied by an image, there can be no notion in the mind". Max Müller argued that, although *some* sort of image was essential, the image of the word alone could carry thought. Galton replied that his own experiments demonstrated that *no* kind of image was absolutely necessary. A thought could be carried by what he described as a 'mental attitude' (cf. 'Thoughts without Words', *Nature*, 1887). Later he commenced a series of ingenious experiments in which he showed that even arithmetical thinking could be accurately performed "solely by imagined scents": e.g., if two whiffs of peppermint are associated with one of camphor, and three whiffs of peppermint are associated with one of carbolic, how many whiffs of camphor are suggested by two of carbolic? In working out such sums he decided whether they should be addition or subtraction by taking up 'the appropriate mental attitude' ('Arithmetic by Smell', *Psychol. Rev.*, 1894). Stout, who acted as subject in some of these experiments, fully endorsed the psychological conclusions thus inferred, and translated Galton's tentative hypothesis into terms of his own doctrine of 'implicit apprehension' (*Analytic Psychology*, 1896, I, pp. 85f.).

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consciousness; out of this antechamber the ideas most nearly allied to the problem at issue appear to be summoned in a mechanical or a logical way, and so have their turn of audience.” Beyond or below all this there is a darker basement, an underground storehouse from which older and remoter ideas can with greater difficulty be hauled up into consciousness. In all this we detect a remarkable anticipation of the theories and methods adopted later on by psychoanalysts like Freud and Jung.[1]

Galton applies the same hypothesis to account for the achievements of genius. The creativity of successful thinkers—great orators, imaginative writers, and inventive scientists—arises largely from the interplay of their conscious and unconscious processes: a flood of relevant ideas is always flowing through the channels of their fertile minds, usually without deliberate exertion or control; and the associations that bring the right idea at the right moment to explicit utterance operate like some curiously logical sorting machine. When they lack this automatic means of selection, such persons are likely to exhibit the phenomena of ‘ morbid imagination ’ so characteristic of the neurotic and the insane. The ability to produce this rich flow of ideas he terms ‘ fluency ’—a specific type of ability, which, it may be remembered, Spearman and many of his research students later on subjected to systematic investigation.

One or two further studies of what Galton termed ‘ morbid imagination ’ may be mentioned, since they illustrate the daring way in which he sought to combine introspective with experimental techniques. In the hope of gaining some insight into the way the fantasies of the insane are built up, he determined, during the course of a morning’s walk, to invest everything he encountered with the attribute of a secret spy. The experiment was only too successful. By the time he had reached Piccadilly, he says, “ every horse on every cab-stand seemed to be watching me either openly or in disguise ”. These persecutory delusions, self-induced, lasted for eight or nine hours, and could be very easily revived, even two or three months later. On another occasion he hung up a picture of Mr. Punch, and treated it as a kind of fetish or idol, mentally ascribing to it all sorts of divine and magical powers. At first these daily rites had no effect, but presently he found himself behaving as though his self-imposed fancies were literally true. An ever-present image stamped itself upon his mind; and round

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[1] A year or two later he developed these ideas still further in the last of his introspective experiments—a study of the influence of involuntary or unconscious processes on voluntary action. Here he drew special attention to the effects of the mental conflict or repression which often operates beneath the surface of ordinary consciousness. There seems to be, he said, a kind of struggle between two ‘ Egos ’. “ The self is by no means one and indivisible. . . Those who study the genesis of dreams will discover plain causes for thoughts that seem to have arisen spontaneously; for the imagination works in obscure depths out of the usual ken of consciousness ” (‘ Free Will: Observations and Inferences.’ *Mind*, IX, 1884, pp. 406–13). Finally, let me commend to those behaviourists who doubt the value of introspection the following remarks, as well as the evidence on which they are based: “ Although philosophers may have written to show the impossibility of discovering what goes on in the minds of others, I maintain the opposite opinion. I do not see why the report of a person on his own mind should not be as trustworthy as that of a traveller in a new country whose landscapes are quite different to anything that we ourselves have seen ” (*Nature*, Jan. 15, 1880, p. 256).

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it were gathered a set of potent emotional associations—all utterly irrational. For weeks after the experiment was over, he still retained a deep and awe-struck reverence for Punch's grotesque figure.

Galton ends his discussion of the effects of association by describing the various ways in which young children seem to acquire their 'early sentiments'—how they start imbibing the likes and dislikes of their parents, and later tend to assimilate first their teachers' views, then the attitude and outlook of their social class and the religion of those around them, and with it the implicit moral ideals that make up what is popularly called 'conscience'. What kind of 'sentiments' each one acquires is thus very largely "a matter of accident"—the outcome of blind association operating under the influence of gregarious and sympathetic 'instincts' similar to those Galton had observed among the wild cattle of Damaraland.[1] The detailed differences, he suggests, would make a fruitful topic for future research. The human models each child comes to admire, the stereotyped conceptions he venerates or derides, his 'working religion' (the word he says, is used "in its larger sense")—all this could be studied by statistical procedures, and the results would shed a flood of light on the problem of motivation. "Here," he concludes, "the power of Nurture seems very great"; it "sets up a kind of atmosphere", which "affects large classes of people in very similar ways", and thus "gives a fallacious sense that these widespread modes of behaviour are natural instincts".[2]

From Nurture he turns to Nature; and this forms the subject of the rest of the *Inquiries*. After casting about for some practicable means of "distinguishing between the effects of the tendencies that are implanted at birth and those that are imposed by the special circumstances of the individual's after-life", he finally hit on the happy idea of making a comparative study of the histories and characteristics of twins. This "new method", he believed, might enable us "to weigh in the scale the effects of Nature and Nurture, and ascertain their respective shares in framing the dispositions and the intellectual abilities of men." He notes the importance of discriminating between different types of twin, and particularly the need to secure comparable data for monovular twins who have been reared apart and binovular twins who have been reared together. His own results, largely anecdotal at this preliminary stage, are reported in some detail. His main conclusion is that "Nature prevails enormously over Nurture when the differences in Nurture do not exceed what is commonly found among persons brought up in the same rank of society."

After this we might expect a full discussion of the inheritance of 'Intellectual Differences'. But the section that bears this heading is tucked away in another portion of the book, and consists merely of a dozen lines referring to his two previous volumes. There are, however, several instructive sections and appendices describing his early attempts to produce standardized tests, such as his calibrated whistle for the upper limit of audible pitch and his geometric series of weights for measuring the delicacy of weight perception. The principles underlying these methods of measurement, he tells us, are quite applicable to other senses and to other faculties. He records the interesting observation that, on an average, acuteness of sense-discrimination is “ highest among the intellectually ablest ”, e.g. among the Fellows of the Royal Society whom he had tested. It was this and similar findings that led Spearman to put forward his well-known theory that ‘general intelligence’ and ‘general sensory discrimination’ are identical and that intelligence itself can best be measured by tests of sensory discrimination.[1]

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[1] For the 'herd instinct' see *Inquiries*, pp. 72f. Darwin had made much the same point (cf. *Descent of Man*, I, pp. 156–167).

[2] It was this rough and ready sketch which suggested to McDougall his attempts to study 'The Development of the Sentiments' (*Introduction to Social Psychology*, chaps. V–IX: his own conclusions were largely based on daily records relating to the development of his own children and their companions). But the whole investigation is well worth carrying out afresh on the lines sketched out by Galton.

The closing sections of the *Inquiries*, like those of *Hereditary Genius*, are concerned with tentative reflections on ‘ the ability of nations ’ and ‘ the influence of man upon race ’—themes which Galton was to develop in much fuller detail later on. In these early speculations, as Professor Darlington points out, “ Galton foreshadows a genetic interpretation of the history and the structure of society ”. He begins by emphasizing “ the vast variety of natural faculty in members of the same race ”—a variety which most commonly results from the “ intermixture of races ” and provides the raw material for selective agencies to work upon. The larger and more heterogeneous the nation, the greater will be the range of variation: the greater therefore will be the chances that such a nation —once it has realized the importance of encouraging and educating its ablest members—will become a ‘ dominant race ’.[2]

Human evolution, we are told, has continued throughout the historical period, and is still going on around us. Hardly any spot upon the earth remains tenanted by its original or earliest stock. The races that now inhabit Egypt and Mesopotamia, Italy and Greece, Australia, South Africa and the United States, are different in physique, in colour, and probably (so Galton believes) in mental and temperamental qualities, from those that inhabited them a few centuries ago. In Western Europe the dominant classes of today are descended from families that were quite unknown to history before the industrial revolution. In North America a group of bold and independent whites, who regarded themselves as a community of equals no longer handicapped by the mediaeval tradition of feudal rights or aristocratic privileges, have, in the course of two or three hundred years, evolved into a nation with a class structure quite as well marked as that of any European society today. Such changes, he contends, cannot be accounted for solely by environmental conditions or solely by genetic conditions, but only by a continual interaction between the two—an interaction which he conceives as a process of cumulative selection from among the immense variety of individuals constantly provided by genetic differentiation. The process of selection is itself open to intelligent study and to intelligent control; and thus " the power which man possesses of influencing the human stock vests a great responsibility in the hands of each fresh generation ". In short, as Professor Darlington puts it, " the nation which takes most thought about its genetical future is the nation most likely to have a future ".[1]

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[1] See for fuller details, *J. Anthrop. Inst.*, XII, 1883, pp. 472f. Galton notes, however, that occupational experience may at times enhance sensitivity. He declares that “ as a rule, men have more delicate powers of discrimination than women ”, though in a later paper he qualifies this premature generalization, saying that he found that with certain senses—e.g. the sense of touch and discrimination of colour—women are on an average superior. It will be observed that Galton's classification of mental processes was largely borrowed from Bain. Bain held that intellectual processes were based on the apprehension of sensations, or of the revived sensations known as ‘ images ’, and were developed mainly by the processes of association. He held too that “ the primary attribute of intelligence is the consciousness of difference, or Discrimination ”. It was this contemporary view that both Galton and Spearman supposed that their experiments confirmed.

[2] He suggests, for example, that the Chinese, once they have “ overcome certain of their peculiar religious prejudices, might make their industrial and social influence felt in many distant regions ”, and perhaps even “ become one of the most effective of the colonizing nations ”.

Galton never tires of reminding us " how new and complex the problem is "; and he insists again and again on the need for scientific research. He ends his book with a practical suggestion as to the ways in which such researches might begin. Two of the most obvious modes of selection arise from variations in health (under which he includes mental defect as well as susceptibility to physical disease) and variations in fertility. He therefore appeals to the " medical profession " to lead the way by exploiting their opportunities for genetical investigations—an appeal which has since been abundantly justified.

Both in the *Inquiries* and in other publications Galton drops frequent hints revealing how keenly he was interested in what is loosely termed the psychology of religion. The first edition of the book included some " Statistical Studies into the Efficacy of Prayer ".[2] Although the objective results were negative, he stoutly defends the subjective value of prayer. " A confident sense of communion with God must necessarily strengthen the heart "; but even agnostics may rejoice in the knowledge that they " have a brotherhood with all that is ".

In the early seventies—ten years before the foundation of the Society for Psychical Research—he became interested in spiritualism, and witnessed several demonstrations given by Dunglas Home (the reputed original of ' Mr. Sludge the Medium ' in Browning's poem) and by other spiritualists. Home's séances were attended by many eminent people of the day—A. R. Wallace and Sir William Barrett (who were convinced), Darwin and Huxley (who considered Home a fraud), George Eliot, Mrs. Browning (who was completely swept away), and Browning himself (who was infuriated). In an early letter to Darwin, Galton writes that he has on more than one occasion attended sittings at (Sir William) Crookes' house, and feels " very disinclined to discredit them: . . . I really believe what they allege—that the people who come as men of science are so opinionated and obstructive that the séances rarely succeed with them ". Many of the details reported by Galton (e.g., the alleged communications from Benjamin Franklin) tally closely with the description given in Browning's poem. Later he appears to have become disgusted with the frequent mixture of fraud and evasion and the " nonsensical twaddle contained in most of the so-called messages ".

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[1] ' The Control of Evolution in Man ', *Proc. Roy. Inst.*, XXXVII, 1958, no. 165.

[2] E.g., calculations show that " members of the Royal Houses, whose longevity is most widely prayed for, have, of all groups, the least average length of life ". As may easily be imagined, all this gave a good deal of pain to his Anglican friends: for (to quote his own quotation from *Hudibras*)

As 'twas said, he scarce received
For gospel what the Church believed.

Accordingly he omitted the whole discussion from the second edition. His own belief appears to have been that " our personalities may be transient but essential elements of an immortal and cosmic mind " (*Inquiries*, p. 196). But he was reluctant to commit himself to any view that had not been empirically tested. As the years went on he endeavoured to make his own eugenic creed—" working for the good of the race "—the basis for a kind of religion.

This, however, was but one of his many minor interests. The establishment of what he called an ' Anthropometric Laboratory '[1] was something far more momentous. It furnished the starting-point, and to a large extent the model, for the many ' psychological clinics ' or ' child guidance centres ' subsequently set up in Britain, America, and elsewhere; it formed the birthplace of the whole ' mental testing ' movement; and it symbolized the beginning of experimental psychology in this country. The laboratory was opened during the International Health Exhibition in London in 1884. When the Exhibition closed, it was moved first to the Science Museum in South Kensington, and then to University College. The College still preserves a few of the large orange posters which announced that an " Anthropometric Laboratory for the Measurement of Human Form and Faculty has been instituted, partly for anthropometric experiment and research, partly to familiarize the public with the methods and uses of human measurement, and partly for those who desire to learn what are their bodily and mental powers or those of their children, or to obtain timely warning of remediable faults in development. Charge, threepence each to those who are already on the register, fourpence to those who are not ".

" The leading idea ", so Galton explained later on in his *Memories* (p. 267), " was that the measurements should ' sample ' a man with reasonable completeness: they should measure *absolutely* wherever possible, otherwise *relatively* among his class fellows ", i.e. by ' ranks ' or ' percentile grades '.[2] Many of the routine measurements were purely physical—tests of bodily strength, measurements of the body and its parts (including the head), observations on hair colour, eye colour, handedness, and the like, and a general note of the examinee's health.

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[1] It will be noted that throughout his published writings Galton deliberately avoids such terms as ' psychological laboratory ' or ' psychological research '. This was partly because in those days the words would have suggested to the general public that the laboratory was intended for ' psychical research ', or, as we should say today, for ' parapsychology ', but chiefly because he disliked the idea of human personality being split into two distinct halves—a mind that is studied by the philosopher or psychologist and a body that is studied by the physiologist or doctor—a ghost loosely coupled with a corpse. ' Bodily ' and ' mental ' were for him two complementary aspects of one individual man; and their investigation formed merely two special departments of a single branch of science, which he preferred to name ' anthropology ', that is the ' study of man ' in the broadest sense of the phrase.

[2] This device was first suggested in *Nature* (1874) and described more fully in *Phil. Mag.*, XLIX, 1875, pp. 33–46.

The rest of the examination was essentially psychological. This was designed to cover three main levels: first, the simple sensory and motor capacities, secondly, associative processes—memory, habit formation, imagery, reaction time, and the like, and finally, what were designated ' higher mental processes '.[1]

For much of his work he had the help of a young American psychologist, J. M. Cattell, who had been studying with Wundt at Leipzig, and acted for a time as Galton's assistant in London. However, in 1888 Cattell was recalled to occupy the first chair of psychology established in the States; and it was principally owing to his introduction of Galton's ideas that mental testing became so popular in that country.[2]

Data collected in this way formed the basis of Galton's next most important work—*Natural Inheritance*, published in 1889. As the title implies, its purpose was to discuss the need for studying the basic principles of biological transmission by scientific methods, and to indicate how this might be done. In many ways his theory of inheritance resembles that put forward by Mendel. In an introductory section he observes that " all living things are in one aspect individual and composite in another. We seem to inherit bit by bit. Inheritance may therefore be described as largely, if not wholly, ' particulate '; and as such it will be treated in these pages." No doubt many human characteristics—height and weight, for instance—exhibit a graded distribution; but each of these, Galton argues, can be regarded as " a fine mosaic with elements too minute to be distinguished ". Here we have a clear anticipation of the principles of multifactorial inheritance, where in Galton's phrase " the multiplicity of quasi-independent elements " combine their effects, and so produce the normal frequency distribution and the varying degrees of family resemblance that we measure by our correlations.

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[1] The scheme and the phrase ' higher mental processes ' were, I believe, due largely to suggestions from Professor Croom Robertson, Sully's predecessor at University College, founder and editor of *Mind* (1876), and a close friend of Galton: (see *Memories*, p. 267, and J. Sully, ' Reminiscences of Professors ', *Univ. Coll. Gazette*, III, 1901, p. 250f.).

Galton displayed his usual mechanical ingenuity in designing suitable instruments and other materials. The ' whistles for determining the upper limit of audible sound ' (referred to above), were invented as early as 1866; ' apparatus for testing the delicacy of muscular and other senses ' in 1883; methods for the ' measurement of character ' were described in the *Fortnightly Review*, 1884; other devices are described in *Human Faculty*. Little progress was made in tests for ' higher mental processes ' until the beginning of the present century.

Within the first two or three years over 9,000 persons were measured at the laboratory, ranging in age from under 8 to over 80. One of the earliest examinees was William Gladstone, the Liberal Prime Minister. Gladstone was very proud of the fact that he took an outsize in hats; and, when the measurements were completed, could not resist saying: " I imagine, Mr. Galton, that is the largest headsize that your laboratory has hitherto encountered ". Galton, we are told, replied: " I fear, Mr. Prime Minister, that you must be rather unobservant ". (Galton himself owned a still larger head.) " And in any case, he added, " I have come to the conclusion that to *assess* ability you must *measure* ability, and not merely measure skulls ".

[2] See J. M. Cattell, ' Mental Tests and Measurements ' (with an Appendix by Galton), *Mind*, XV, 1890, pp. 373–380.

V. STATISTICAL TECHNIQUES

Regression. Among statistical psychologists and indeed among statisticians generally Galton's name and reputation are commonly linked with the concept of correlation. This does him less than justice. But let us take this particular technique as a starting point and see how the concept arose.

Mendel, it may be remembered, was interested chiefly in discontinuous characteristics, such as flower colour. Galton, on the other hand, devoted his attention mainly to graded characteristics, no doubt because almost all the measurable characteristics of human beings are of that type: discontinuous characteristics (like eye-colour) which are subject to what he called 'alternative heritage' he examined more briefly. The clue to inheritance was to be found, so he believed, in the different amounts of family resemblance exhibited by persons having various degrees of kinship; and accordingly he cast about for some method of measuring the degree of resemblance. By a curious coincidence his earliest investigations, like those of Mendel, were concerned with inheritance in peas: in Mendel's experiments they were edible peas; in Galton's they were sweet peas.

On measuring the size of seeds for both mother and daughter plants, he discovered that, when the mother plants had produced large seeds, the daughter plants also produced large seeds, but the "amount of the increase over the average" proved to be much smaller. In general, so it appeared, "for every increase of one unit on the part of the parent seed there is a mean increase of only one-third of a unit in the filial seed". The daughter plants in fact tended to revert to the general mean.[1] He reached much the same result when, a few years later, he compared the heights of parents with their adult offspring. On taking a batch of tall fathers—e.g., those whose heights was three inches above that of the average adult—and then measuring the height of their adult sons, he found that the mean height of the latter was only one inch above the general average. He termed this reduction in the mean height of the second group its 'regression', and the ratio of the two deviations—in this case 1/3—the 'regression coefficient'.

Galton was fully aware that this mode of arguing involved certain assumptions which required empirical confirmation, namely, that the fraction remains constant for all the groups (i.e., that the 'regression' is 'linear', as we should now say), and that the 'dispersion' of the various sub-groups about their respective means is the same (i.e. in Pearson's terminology, that the 'arrays' are 'homoscedastic'). But he regards his earlier data and figures as offering illustrations rather than proofs. His theoretical demonstration proceeded not by considering resemblances or by calculating correlations, but reasoning along the lines of what Fisher has taught us to call an 'analysis of variance'. As his novel ideas were continually evolving throughout these earlier researches, it is not surprising if his nomenclature and symbols change from time to time in a somewhat confusing way and there are occasional slips in his formulae. The consequence is that the modern student too often gives up the attempt to follow his rather groping arguments. I shall therefore summarize the gist of his demonstrations first of all in terms of symbols that are a little more familiar.

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[1] The experiments were described in a lecture delivered at the Royal Institution in 1877: the diagram in his notes illustrating the results obtained must contain the first regression line ever drawn. The peas were measured by arranging 100 in a row, and then measuring the length of the row in inches. Similar results were also observed for the weights of the seeds.

The problem in his first paper is essentially the problem of prediction: given, for example, the height ($m \pm x_i$) of a particular individual, i (where m denotes the general mean), what will be the most probable height ($m \pm y_i$) of his brother? Galton assumes that this can be found from the equation

$$y_i = w_{yx} x_i, \tag{1}$$

where w, he says, is " what I call the ratio of regression ". Thus $w_{yx} = y_i/x_i$; and, he adds, " I shall presently show that the value of y_i/x_i is constant for all statures in the same degree of kinship."[1]

His basic postulate is that " natural peculiarities are due to two different causes: the one is Family Likeness, the other Individual Variation ". The word 'peculiarity' is used in a technical sense " to signify the difference between the amount of any faculty (or other characteristic) possessed by a man and the average of that faculty possessed by the population at large," i.e., it signifies an individual 'error' or deviation from the mean (m). "Family likeness," he says, must be " due to a *common cause* "—to a " factor of stability."[2]

Let us therefore write g for the common or general factor, and s_x and s_y for the individual or specific factors, both being uncorrelated with g. Then

$$x = g + s_x \tag{2}$$

$$y = g + s_y. \tag{3}$$

Squaring and taking averages we have (since $\Sigma gs = 0$)

$$\sigma^2_x = \sigma^2_g + \sigma^2_{sx}, \text{ and } \sigma^2_y = \sigma^2_g + \sigma^2_{sy}. \tag{4}$$

Thus, the observed dispersion in either sample is (as Galton puts it) " compounded of two superimposed and independent systems of dispersion ", i.e., in his own symbols, $q^2 = b^2 + c^2$: " in other words, q corresponds to the hypotenuse of a right-angled triangle of which the other two sides are b and c " (the 'law of vector addition', as Maxwell Garnett later termed it).

From the theory of correlation we have

$$r_{xy} = \frac{\Sigma x^2}{\sqrt{(\Sigma x^2 \, \Sigma y^2)}} = \frac{\sigma^2_g}{\sqrt{\{(\sigma^2_g + \sigma^2_{sx})\,(\sigma^2_g + \sigma^2_{sy})\}}} \tag{5}$$

$$= \frac{\sigma^2_g}{\sigma^2_g + \sigma^2_s} = w_{xy} = w_{yx}, \tag{6}$$

when (as in the case of correlation between brothers) the two variances are equal.

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[1] 'Family Likeness in Stature', *Proc. Roy. Soc.*, XL, 1886, pp. 50f. (section headed 'Regression'). I have substituted y_i/x_i for Galton's x'/x.

[2] *Natural Inheritance*, pp. 9f. and 194f. In this later publication the treatment and the formulae are much the same as in the earlier paper, but the theoretical discussion is fuller and clearer.

If, however, we assume that the *given* measurements contained no specific factor, i.e., that $x_i = g_i$, then we obtain from eqn. (5)

$$r_{xy} = r_{gy} = \sqrt{\left(\frac{\sigma^2_g}{\sigma^2_g + \sigma^2_s}\right)}. \tag{7}$$

Galton seems at times to confuse the two cases.

His other illustrative problem is the prediction of the height of adult sons (y) from that of their midparents (x). In this case therefore we have

$$w_{yx} = r_{xy}(\sigma_y / \sigma_x) = \frac{\sigma^2_g}{\sigma^2_g + \sigma^2_{sx}}. \tag{8}$$

Let us now turn to Galton's own deductions from his first hand observations. Instead of the standard deviations σ_x or σ_y, he takes the ' probable errors ', which he designates p (*Family Likeness*, p. 50). He estimates them by calculating the ' quartile values ' (q). In his main equations, however, he employs p for *both* the variabilities. To avoid the consequent confusion I shall therefore add the suffixes x or y. The data that he uses to illustrate and verify his formulae consist of (1) " heights of brothers more than 24 and less than 60 years of age ": (these he obtains from just under 300 families " containing in the aggregate 783 brothers "); (2) " heights of 205 couples of parents, with 930 children of both sexes ": here he first "transmutes the female measurements to their adult equivalents" (by multiplying the former by 1·08), and then takes for the measurement of the mid-parent " the imaginary mean of the father and mother " (*loc. cit.*, p. 53). His measurements for adult males range from 61 in. to 75 in. with a mean at 68 in. (or a fraction over). In tabulating the frequencies he subdivides this total range into 15 sections of one inch each, with deviations of $-7, -6, \ldots, -1, 0, +1, \ldots +6, +7$ in. above or below the general mean.

Consider first of all just one individual. John Brown, say, whose height is 71 in. ($x_i = +3$ units): what will be the most probable height of one of his brothers? To determine this we should calculate the average of all the brothers in the Brown family: the heights of the individual brothers will of course differ somewhat from each other; but the variation will be decidedly less than the variation in the whole population. " The science of heredity," however, " is concerned with groups rather than with individuals." Let us therefore consider the brothers, not of a single individual, John, in a single family, the Browns, but the larger group formed by the brothers of *every* individual whose height is the same to the nearest inch, viz., $+x_i$ units. Such a ' composite family ' Galton calls a ' co-fraternity '. He now considers measures for three types of variation: (i) the variation of all adult *individuals* about the mean of the *general* population to which they belong: he calls this p; (ii) the variation of the *means* of the several co-fraternities about the mean of the general population: he calls this d; and (iii) the variation of the *individuals* in a given co-fraternity *about the mean* of that co-fraternity: this he calls f. Clearly, expressed in current terminology, what he is really doing is analysing (i) the variance of the total population into (ii) the variance *between* co-fraternities, and (iii) the variance *within* co-fraternities.

All the individual deviations may, he says, be regarded as ‘errors’ obeying the exponential or ‘normal law of error’; and “from a well-known property of the law of error we obtain an equation which assumes the form

$$p^2 = d^2 + f^2\,;\text{''} \tag{9}$$

in other words, he treats the variances as additive. But, he continues, $w_{yx} = y_i/x_i$ (eqn. 1): i.e. the regression coefficient is by definition the ratio of the expected deviation of a brother of John to the actual deviation of John himself, or rather the average of all such ratios. Therefore $w^2{}_{yx} = d^2/p^2{}_x$, i.e., $d^2 = w^2{}_{yx}p^2{}_x$; or dropping subscripts because $p^2{}_x = p^2{}_y$, $d^2 = w^2p^2$. This gives us a theoretical equation which we ought to be able to verify from our empirical data, viz.,

$$p^2 = w^2p^2 + f^2, \tag{10}$$

that is,

$$w^2 = \frac{p^2 - f^2}{p^2} = \frac{c^2}{c^2 + b^2}, \tag{11}$$

where c^2 may be regarded as representing the variance of the common factor, and b^2 that of the specific factor.[1] Thus, as Galton says, $c^2/(c^2+b^2)$ “is the value of the Fraternal Regression”[2]. His equation 11 is our equation 8.

Galton now proceeds to check this result from his own data. (i) From the measurements for brothers we have $p = 1{\cdot}71$, $f = 1{\cdot}27$, and $w = 2/3$ (determined by averaging the regressional ratios for the different co-fraternities). Substituting in eqn. (10) we obtain

$$(2/3 \times 1{\cdot}71)^2 + 1{\cdot}27^2 = 2{\cdot}929 = 1{\cdot}71^2 \text{ (approx.)}.$$

(ii) From the measurements for mid-parents and their offspring we have for the variability of the former (assuming, as Galton does, that mating is random) $p^2{}_x = 1/2 \times p^2{}_y = 1{\cdot}46$ or $1{\cdot}21^2$, $f^2 = 1{\cdot}50^2$, and $w_{yx} = 2/3$ as before. Equation (10) should now be written

$$w_{yx}{}^2 p_x{}^2 + f^2 = p^2{}_y$$

or $(2/3 \times 1{\cdot}21)^2 + 1{\cdot}50^2 = 2{\cdot}911$, which again is approximately correct. To determine the converse regression, w_{xy}, Galton gives the equation[3]

$$p^2{}_x w_{yx} = p^2{}_y w_{xy}, \tag{12}$$

from which he obtains $w_{xy} = 1/3$.

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[1] *Loc. cit.*, pp. 50, 51; *Natural Inheritance*, p. 114.

[2] *Natural Inheritance*, p. 127. As originally printed the value given for the Fraternal Regression is $\surd\{c^2/(c^2+b^2)\}$ both here and on p. 70. But in Galton's own copy the radical has been scratched out, and the equation on p. 70 altered, in Galton's own handwriting, to $c^2/(c^2+b^2)\,x$. The correct value appears in Appendix C, p. 224.

[3] I have substituted my own notation; Galton here (*loc. cit.*, p. 57) writes c^2 for $p^2{}_x$, which is doubly confusing, since c has already been used with another meaning. It should be added that the values obtained for the regressions differ somewhat from those more recently obtained from larger and more carefully chosen samples of the total population. Galton is aware that b^2 cannot be identified with f^2, as eqn. (11) might at first sight suggest. For brothers he estimates b as about $1{\cdot}06$ (‘Family Likeness’, p. 59). On this basis we should have $w = 1{\cdot}80/(1{\cdot}80 + 1{\cdot}12) = 0{\cdot}62$.

The student will find it highly instructive to compare Galton's treatment of the analysis of variance with that adopted in more modern textbooks (c.f. for a general statement of the problem at its simplest, Tippett, *Methods of Statistics*, Tables XXIV and XXXVIII (pp. 94 and 122), and for the correlation between brothers, Fisher, *Statistical Methods for Research Workers*, Table 39, p. 213. We may note that Fisher's formula for the correlation $\rho = A/(A+B)$ is virtually the same as Galton's (eqns. (6) and (11) above).

Galton then turns to consider the bivariate distribution. In his *Memories* (p. 302) he relates how, while waiting on the platform for a train, he started poring over the tabular diagram for the heights of parents and their children. It then struck him that the contours joining points of equal frequency " ran in concentric ellipses ". He started brushing up his knowledge of conic sections; but in the end he submitted his problem to Hamilton Dickson, a Tutor in Mathematics at Cambridge. Dickson at once sent him the formal solution,[1] adding that most of his mathematical class were able to solve it. Galton tells us that he " never felt such a glow of respect towards the wide sway of mathematics as when (Dickson's) answer arrived confirming my laborious statistical conclusions with far more minuteness than I had dared to hope ". This perhaps is scarcely the feeling the modern student experiences when he tries to follow Dickson's somewhat tortuous exposition.[2]

Galton has found that the offspring of parents (or midparents) whose deviation is x will vary about a mean of $y = w_{yx}x = (r\sigma_y/\sigma_x)x$ with a variance of $\sigma^2{}_y\,(1-r^2)$, and that their frequency distribution will obey the ordinary normal law, i.e. (in the ordinary notation) that, if there are m offspring of the n parents having a deviation of x, the number of offspring having a deviation of y will be

$$z = \frac{mn}{\sigma_y\,\sqrt{(2\pi)}\,\sqrt{(1-r^2)}}\,\exp - \frac{1}{2\sigma_y{}^2(1-r^2)}\left(y - \frac{r\sigma_y}{\sigma_x}\,x\right)^2. \tag{13}$$

But

$$n = \frac{N}{\sigma_x\,\sqrt{(2\pi)}}\,\exp - \tfrac{1}{2}\,\frac{x^2}{\sigma_x{}^2}, \tag{14}$$

where N is the number in the total population. On substituting for n in the preceding equation, we then have for the exponent (omitting the factor $-\frac{1}{2}$)

$$\frac{x^2}{\sigma_x{}^2} + \frac{1}{\sigma_y{}^2(1-r^2)}\left(y - r\frac{\sigma_y}{\sigma_x}\,x\right)^2. \tag{15}$$

This, in modern notation, is what Dickson calls " the exponential of the exponent which appears in the value of z in the equation of the surface of frequency " as formulated by him (without explanation) at the outset of his proof.

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[1] It is printed as an appendix to ' Family Likeness' (*Proc. Roy. Soc.*, XL, pp. 63–66) and to *Natural Inheritance* (pp. 221–224). The value evidently assumed for r^2 (which does not appear explicitly in Dickson's paper) is $w_{yx} \times w_{xy} = 2/3 \times 1/3 = 2/9$, i.e. $r = 0{\cdot}471$.

[2] Galton's problem, as treated in this appendix (but with his theoretical values substituted for the observed and eqns. (11) and (17) for Dickson's equations), forms an excellent way of introducing the elementary student to the whole subject of correlation and factor analysis.

In his initial equation (1), however, he substitutes Galton's numerical values for the various constants; his later eqn. (7) gives the formula in general terms, putting $\tan\theta$ for the regression coefficient $r\sigma_y/\sigma_x$ in eqn. 15 above. On simplifying equation 15 and changing to the relative frequency (or probability) we obtain

$$z=\frac{1}{2\pi\sigma_x\sigma_y\sqrt{(1-r^2)}}\exp-\frac{1}{2(1-r^2)}\left(\frac{x^2}{\sigma_x{}^2}-\frac{2rxy}{\sigma_x\sigma_y}+\frac{y^2}{\sigma_y{}^2}\right). \tag{16}$$

which is the familiar equation for the normal correlation surface and was not in fact explicitly given by Hamilton Dickson.

Dickson also calculates the equation to the ellipses " when referred to their principal axes ". This involves rotating the axes of the coordinates so that the coefficient of the product term xy is zero, i.e. so that

$$\frac{X^2}{c^2}+\frac{Y^2}{b^2}=\text{constant}. \tag{17}$$

He obtains as approximate values $c^2=7$ and $b^2 2$. Once again the essential equations are not stated, namely, in modern notation, $\sigma_X{}^2+\sigma_Y{}^2=\sigma_x{}^2+\sigma_y{}^2=1+1/2$; $\sigma_X{}^2\sigma_Y{}^2=\sigma_x{}^2\sigma_y{}^2(1-r^2)=1/2\times(1-2/9)$. From these we obtain

$$\sigma_X{}^2=7/6 \text{ and } \sigma_Y{}^2=2/6. \tag{18}$$

Correlation. Meanwhile Galton had encountered what seemed at first sight a very different problem. If you want to produce an adequate specificaton of any particular individual with scientific accuracy the ideal procedure would be, not to pick out his most salient peculiarities after the fashion of the literary biographer, but to measure all his distinctively human characteristics. But that is clearly impracticable. How then are the most essential to be selected? In France in order to describe, classify, and identify criminals by their physical characteristics, Alphonse Bertillon[1] had recently put forward a scheme based on twelve body measurements—height, length and breadth of head, length of forearm and of middle finger, and the like, all supplemented by a *portrait parlé*, with photographs and a list of distinguishing marks such as scars and moles. Galton, however, believed that several of Bertillon's measurements would vary so closely with each other that only a few would be really informative. Obviously, if you have measured Arsène Lupin's left leg, you don't need to measure his right leg; and if you measure his height, is it necessary to measure his legs or his arms at all? The question could readily be answered, so Galton argued, if we could only devise some means of assessing the degree of concomitant variation—or, as he termed it, the ‘ co-relation ’—between height, arm-length, head-length, and so on.

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[1] Bertillon (b. 1853), originally a teacher of French in Britain, subsequently obtained a post as filing clerk at the Préfecture in Paris. Ethnologists had already made free use of anthropometric measurements; and, after the Franco-Prussian war, both Prussian and French armies became interested in their application. Bertillon, like Galton, was convinced that such methods had both scientific and practical possibilities. His scheme was tentatively adopted by the Préfecture in 1882; and in 1888 he was appointed Chief of the Service of Judicial Identity. (See A. Bertillon, *Signaletic Instructions, including the Theory and Practice of Anthropometrical Identification*, Engl. trans., 1896.) Meanwhile, a simplified version of his 'metric system of identification' was introduced at Scotland Yard; the British procedure is described by Dr. Garson (who was in charge of it) in *J. Anthrop. Inst.*, XXX, 1900. It should be noted that, in opposition to Galton, Bertillon and Garson were agreed that " the measurements for the different characters would be independent ".

However, the problem is of much wider importance than this. As Galton points out, both the term and the concept are of frequent occurrence in biology.[1] Yet “ no previous attempt had been made to define it clearly or to measure its degree ”.[2] The fact that, in Darwinian phrase, many ‘ variations ’ were closely ‘ correlated ’ was evident from a study of the data he had already collected in his anthropometric laboratory at South Kensington. Thus, if a man’s height is 1 inch over the average, his forearm will be about ¼ inch longer than the average : if his forearm is 1 inch over the average, then he will be taller than the average, not by 4 inches, but by 2½ inches. As expressed by these two values, however, the relations “ are not numerically reciprocal ”; and the regression coefficient no longer serves. What is needed is a single index-figure, varying between zero and unity.

The solution, he suggests, is to “ transmute ” the raw measurements of stature, forearm, weight, and so forth (which will be in inches, pounds, or the like) “ into units determined by their respective scales of variability . . . namely, the probable error ”. When this is done “ the direct ratio and the converse are identical, viz., for height and forearm 0·8 to 1·0 ”. This therefore is the device he proposed to adopt as a measure of the co-relation. In his autobiography he relates how vividly he recalls the circumstance in which he first grasped this important generalization. “ It was in the grounds of Naworth Castle ” (the baronial residence of the Howards, the famous Earls of Carlisle, who were remotely related to him.)[3] There “ an invitation had been given to ramble freely. A temporary shower drove me to seek refuge in a reddish recess in the rock by the side of the pathway. Here the idea flashed across me, and I forgot everything else for a moment in my great delight ”. As Pearson remarks, “ that ‘ recess ’ deserves a commemorative tablet ”.

He found that for nearly all the body measurements advocated by Bertillon the correlations were surprisingly high—ranging from 0·70 (for height and middle finger) to 0·90 (for height and length of lower leg). The correlations furnished by the head measurements, however, both with each other and with the remaining characteristics, proved to be comparatively low (0·35 to 0·45). His statistics thus fully confirmed the ancient maxim, *ex pede Herculem*—if you can measure just one part of a body or a corpse, then you can reconstruct fairly accurately the measurements for the rest—except, significantly enough, for the skull. From the foot alone you can tell whether your man is a giant like Hercules, or a dwarf like Tom Thumb, or what his approximate measurements must be if he is intermediate between the two. In short, underlying all the various measurements of the body lies a " common cause ", or, as we now should say, a " general factor ", more or less modified by the effects of certain more or less " specific factors ".

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[1] See C. Darwin, *Descent of Man*, 1871, I, pp. 64f., section on ‘ Correlated Variation ’, and the glossary and index to his *Origin of Species*. The term was originally introduced by Cuvier.

[2] F. Galton, ‘ Co-relations and their Measurement, chiefly from Anthropometric Data ’, *Proc. Roy. Soc.*, XLV, 1888–9, pp. 135–145, opening sentence.

[3] The history of the castle, twelve miles north-east of Carlisle, is most closely associated with “ belted Will Howard ”, who figures in Scott’s *Lay of the Last Minstrel.*

He goes on to indicate the obvious merits of the correlation coefficient for investigations into heredity. Here again, as in studying the resemblance between adult brothers, the variabilities will ordinarily be the same for both correlated groups. In such cases the two regressions will be identical; and, as indeed he had stated in *Natural Inheritance* (p. 132), " the regression then provides a convenient and correct measure of family likeness ". But, when comparing the stature of adult children with that of their ' midparent ', he had already encountered an instance in which the variabilities differed; and of course the same situation arises when we wish to compare measurements of parents with those of children who are not yet fully grown. Evidently, therefore, the new coefficient, which in point of fact is the geometric mean of the two regressions, is the most general form of index-figure; and it is available for all conceivable cases.

This short but epoch-making paper is concerned merely with the theory of the subject. He notes that the detailed results incidentally obtained " are of interest especially in connexion with M. Bertillon's system "; but with this and other practical applications he proposes to deal elsewhere.[1] The most succinct statement of the obvious corollary is contained in his *Memories* (p. 251). " The incorrectness [of the Bertillon system]," he says, " lay in treating the measures of different dimensions of the same person as if they were *independent* variables, which they are not; thus the chances against a mistake in identification have been enormously overrated." Bertillon, in filing his records, sorted each criminal into three broad classes for each measurement—below average, medium, and above average; he then assumed that with n measurements this would yield 3^n different pigeon-holes or compartments. But this is a manifest fallacy. Almost all the persons of medium height will have arms and legs of medium length; similarly for those whose height is above or below average. Thus in respect of many of his compartments the anthropometer will find himself in the position of Old Mother Hubbard. Consequently, what is really needed is a method of classification based on a set of *uncorrelated* measurements.[2]

Factor Analysis. In the books and papers I have cited the reader continually comes across pregnant hints of highly original notions and techniques, many of which were taken up and developed by statisticians and statistical psychologists during the next few decades.[1] In *Natural Inheritance*, for instance, Galton suggests *combining* measurements for the same characteristic from several relatives, duly weighted, to secure a better prediction of the corresponding measurement for the particular individual in question—the problem of multiple correlation. And his later paper on 'Co-relation' concludes with a brief reference to the possibility of combining measurements for different characteristics obtained for the same individual with a view to securing a more accurate estimate of the degree to which one particular variable is correlated with the joint effect of *n* other variables. He envisages two cases: first, that in which the initial measurements are correlated, and secondly that in which they vary independently. Indeed, as Pearson observes " by 1889 Galton had completed his theory of normal bivariate classification; . . . the next stage was the development of multivariate classification . . . This he endeavoured to reach by a short cut ", but here he was severely handicapped, because " for him the geometry of *n* dimensions remained a closed book " (*op. cit. sup.*, II, pp. 380f.).

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[1] Cf. *J. Anthrop. Inst.*, XVII, pp. 346f, and XX, pp. 198f.

[2] The argument is perhaps most clearly stated by Edgeworth in his criticism of Bertillonage (see this *Journal*, II, p. 102 and refs.).

One major problem was continually at the back of his mind—how to determine the underlying common factors. In the paper just cited he argues that correlation is itself " the consequence of the variations in the two organs being due partly to a *common cause* ". Could a method be found for establishing such causes objectively, then, he believed, we should be able to reach a sound scientific procedure for classifying both the persons and their characteristics. As we have seen, like so many of his relatives, Galton was an ardent believer in classification. But to be genuinely scientific a classification, he considered, should not be just a neat, plausible, quasi-scholastic scheme, excogitated by armchair reflection: it should be empirical, causal, and quantitative. The principles on which such classifications were to be based—the *fundamenta divisionis*, as the logicians would say—should be independent of each other, and, wherever possible, identifiable with the main contributory or ' partial ' causes, i.e., with what would today be termed the ' orthogonal components ' or ' factors ' that contribute most to the variance. In general he recognized three kinds of classificatory principles—generic, specific and individual. Thus, as he puts it, " Personal Forms may be divided into Types, Subtypes, and Deviations."[2]

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[1] He was always shy of committing himself to explicit accounts in print until his ideas had been objectively tested, but ready to expatiate on them in conversation: hence many of the far reaching hints buried in his various publications have been overlooked or misinterpreted by those who did not enjoy a more direct access to the highly ingenious ideas with which his fertile mind was always filled. For an illuminating discussion of these and other implications of Galton's correlational techniques see E. A. Peel, *Brit. J. educ. Psychol.*, XXIV, 1954, pp. 9–16.

[2] *Natural Inheritance*, pp. 25f. This threefold division re-emerges under various names again and again. It was evidently suggested by the Linnaean (or rather Aristotelian) scheme of genera, species, and individuals—terms which Galton himself uses in working out his classification of finger prints (cf. ' Patterns in Thumb and Finger Marks; Their Arrangement into Naturally Distinct Classes, and the Resemblance of their Classes to Ordinary Genera ', *Phil. Trans.*, CLXXXII B, 1891, pp. 1–23). Similarly, as we have seen, in discussing the inheritance of mental traits, he distinguished ' general abilities ', ' special aptitudes ', and ' individual peculiarities '.

The key to the mathematical solution of the problem was really contained in Hamilton Dickson's discussion of Galton's contour ellipses. It consisted in “ referring the ellipses to their principal axes ”, since the principal axes are necessarily orthogonal. The very shape of the ellipses obtained by plotting the data for body measurements—their conspicuous length and their constricted breadth—were in Galton's view clear evidence of the overriding importance of the factor common to both the correlated groups, namely, the hereditary tendency shared by parents and children in one instance and by the pairs of brothers in the other.[1] He went on to show how this common causal factor appeared and re-appeared, though in a progressively attenuated degree, in later generations and in remoter relatives. “ The process throughout,” he says, “ is one of proportionate dilutions ”: even the nearest relatives are “ not entirely of the same blood ”, but “ the blood is, as it were, highly ‘ saturated ’ with the inheritable ingredient; as the kinship becomes more and more remote, so the saturation diminishes.” He suggested a rule for predicting its probable or average amount. The parent-and-child regression was 1/3; the brother-to-brother regression was 2/3. Hence, he argued, since a nephew is the son of a parent's brother, the uncle-to-nephew regression would be $1/3 \times 2/3 = 2/9$; similarly the grandparent-to-grandchildren regression would be 1/9, and the cousin-to-cousin only 2/27. In this way it became possible to construct a theoretical set of regression coefficients to represent what he called ‘ the hierarchy of kinship ’—a ‘ matrix ’ (as we should now say) of ‘ rank one ’. His vivid metaphors seemed at the time to supply a convenient terminology for expressing the new ideas. Younger readers, unfamiliar with this early history, have frequently been puzzled by the rather odd and out-of-the-way terms—‘ saturations ’, ‘ hierarchies ’, and so forth—which no longer seem self-explanatory.

Galton attempted to generalize his chief results in the form of a ‘ law of ancestral inheritance ’. At its simplest it states that correlation between a given individual and a relative at the *n*th remove is r^n. If, as he at one time suggested, we take $r = \frac{1}{2}$, then the correlation between a parent and his child will be $\frac{1}{2}$; between the same person and his grandchild $\frac{1}{4}$; and so on. A similar rule can be deduced from the Mendelian theory of multifactorial inheritance: if for simplicity we ignore the possible effects of dominance, assortative mating, and various other minor complications, we may say that a parent passes on to a child half his or her genes: as a result parent and child (and also two sibs) have half their genes in common. Similarly grandparent and grandchild (or uncle and nephew) have a quarter of their genes in common; and generally at each remove the number of genes in common is halved.[1]

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[1] In both his anthropometric and his psychological studies Galton was always more interested in ‘ general factors ’ than in the more specific. At that date indeed, so far as psychology was concerned, it would have been unnecessary to adduce arguments in support of innate ‘ *special* abilities ’: they were already fully recognized by current faculty theory. On the other hand, the notion of an innate ‘ general ability ’ was a novelty. Spearman, one of his early followers, even went so far as to deny the existence of ‘ special abilities ’ altogether, and was content with the two types of factor revealed in this early analysis.

The other problem which Galton brought to the fore was, as we have seen, the analysis not of single correlations for a single trait, but of a whole table of correlations, for a series of traits. This clearly entails extending Dickson's notion of using principal axes from two dimensions to three or more. Such an extension would seem fairly obvious to an elementary student of mathematics, once the problem had been put to him. Edgeworth did in fact extend the method to three dimensions, and worked out the results for three physical traits, taking Galton's correlations as his starting-point and applying the "well-known rules for transformation to principal axes," as given in every school textbook on solid geometry. Being strictly " independent " (i.e. uncorrelated) these " hypothetical characteristics ", he argued, would supply a far better basis for classification than Bertillon's method of taking the observed measurements just as they stand. We should then begin by classifying each individual according to ' Characteristic No. 1 ', which, as he rightly says, is a ' factor for general body size '; we should next cross-classify them by distinguishing those who are disproportionately long or short in arm or leg. This gives us $3 \times 3 \times 3 = 27$ sub-classes. Pearson made the whole thing completely general by extending the basic idea to the case of *n* intercorrelated variables, and proposed its application to the very extensive set of bodily measurements collected by the Bureau of Identification at Scotland Yard. After this it was a very natural step to apply the same techniques (with suitable modifications) to measurements obtained with mental tests.[2]

By this time, however, Galton himself had become convinced that, for the objective identification of persons, fingerprints would provide a far surer method than body measurements. In a long series of researches, reported in over a dozen memoirs published between 1891 and 1902, he was able to establish three main conclusions. First, " the pattern of a finger-print remains unchanged throughout life ". Secondly, they admit of practical classification, or, as he puts it, of being ' lexiconized '. Thirdly, " the variety of pattern is great enough for mistakes to be beyond the bounds of probability ". He calculates that with his scheme the number of different patterns will be of the order of 2^{36}—roughly

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[1] For a comparison between theory and observational results see Burt and M. Howard, ' The Multifactorial Theory of Inheritance ', this *Journal*, IX, esp. Table IX, p. 117. In developing his general theory Galton was largely influenced by his hypothesis of particulate inheritance. However, quite often in discussing his law he appears to have supposed that different ancestors handed on their traits separately. Pearson, who wholly rejected the notion of particulate inheritance, introduced other erroneous modifications; see his discussion in *The Grammar of Science*, 1900, pp. 475f., and especially the table on p. 495.

[2] I have described in fuller detail the history of the subject (with relevant references) in the paper already cited (this *Journal*, II, pp. 98f.). So far as the study of body-build is concerned, the most comprehensive investigation hitherto carried out is that undertaken for the Royal Air Force in which correlations between measurements for nine physical traits obtained from 2400 entrants were factorized. The results furnished a theoretical basis for what in those days was popularly termed ' somatotyping ': (cf. C. Banks and C. Burt, 'A Factor Analysis of Body Measurements for British Adult Males ', *Ann. Eugen.*, XIII, 1947, pp. 238–256).

68 thousand million; the population of the world he assessed at about 1·7 thousand million. Hence the odds are about 40 to 1 against the pattern found on a particular finger in one person being found again in a second person. He concludes his argument by reminding the reader that in the story of Jezebel it was said that, besides her skull, only " the palms of her hands and the soles of her feet were left, so that no man might say: ' This is Jezebel.' " But, he adds, " these are the very remains by which her corpse might have been most surely identified."[1]

VI. Practical Applications

The later years of Galton's life were devoted more and more to lectures and articles on the practical implications of the views he had thus developed. Since human achievements depend partly on education and partly on hereditary transmission, it is essential, so he maintained, to apply modern scientific techniques to the improvement of both. For this purpose the first step, he held, must be to increase our knowledge of each contributory process by means of scientific research; and the most obvious place for carrying out such investigations is the school. This indeed he had already realized long before he opened his Anthropometric Laboratory.

" As each hospital fulfils two purposes—to relieve the sick and to advance the science of medicine, so," he argued, " the object of the school should be, not only to educate, but also to promote the science of education." The school was itself an anthropometric laboratory, " ready and waiting for anthropological researches to be carried out there . . . If," he says, "a few masters were willing to codify in a scientific manner their large experience of boys, and to assess and compare their intellectual and moral qualities, classify their temperaments, and describe them as a naturalist would describe the fauna of some new land, what excellent psychological work might be accomplished ". In a later paper, addressed to school teachers and school doctors, he put his proposals in a nutshell: " Anthropometry is the art of measuring the physical and mental faculties of a human being. It enables a short-hand description of any individual to be obtained by recording measurements of a sample of his qualities. Properly chosen, these will define his bodily proportions, his health, strength, and energy, his intellectual capacities and moral character, and will substitute concise numerical values for verbose and disputable estimates. Anthropometry thus furnishes the readiest method of estimating whether a boy is developing normally or otherwise . . . But no programme for anthropometry in the school can be considered complete unless it also provides for the collection of further data during the pupils' after-lives. We need to know what is its prophetic value. How far can performance in youth foretell success or failure in after life?" Accordingly he suggested that a kind of school record card or personal file should be kept for each child, to be filled up every leap-year on February 29. This would at once assist both educational and vocational guidance. At the same time plans should be laid for supplementing the case-histories begun during childhood by after-histories carried out at regular intervals during adult life.[1]

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[1] *Finger Prints*, 1893 (Macmillan). In 1893 Mr. Asquith, then Secretary of State for Home Affairs, appointed a committee (consisting of Major Griffiths, the Inspector of Prisons, Mr. Macnaghten, the Chief Constable of the Metropolitan Police Force, and other persons) to inquire into the comparative merits of " the anthropometric system of classification and identification in use in France and the suggested system of identification by means of finger marks ". The committee reported that they were " much impressed by the excellence of Mr. Galton's system ", and " think his conclusions may be entirely accepted ". The representatives of the police force were, I suspect, strongly influenced by the fact that a criminal would be very apt to leave his fingerprints on the scene of the crime; he would not leave his body measurements there. The first recorded case in which finger prints were used in actual evidence was the trial of Harry Jackson for burglary, at the Central Criminal Court on September 13, 1902; and in the following year Galton's argument was strikingly confirmed by the curious coincidence that in the United States two " Will West's " were found with the same body measurements, but with very different patterns in their finger prints.

As a result of the experience gained from the earlier work in schools, Galton, when President of the Anthropological Section of the British Association, was led to propose an anthropometric survey of the British Isles. The first investigations were confined almost entirely to the collection of physical data.[2] However, a quarter of a century later, in 1903, he suggested repeating the survey, and urged that this time it should include mental as well as physical measurements, McDougall was appointed secretary of the Psychological Committee; and for the purpose of the inquiry initiated a series of researches in his laboratory at Oxford. Their chief aim was to develop and standardize tests of general intelligence and other abilities for use in the schools through which the surveys were to be carried out. McDougall's research students—William Brown, J. C. Flugel, H. B. English, and myself—set to work on this project with much encouragement from Galton. Spearman, who had just returned from studying experimental psychology under Wundt, also came to Oxford, and joined in the scheme.

During the next ten years or so the researches thus started were continued in London, Liverpool, and elsewhere. Then, after prolonged appeals by Sully and Galton, the London County Council resolved to appoint a school psychologist of its own. Since the post was partly a research post, we were able to put into execution many of the plans that Galton had long ago proposed. With the help of research students (chiefly teachers) trained in the various University departments, we made regular surveys of typical boroughs; we hunted up identical twins who had been reared apart; we selected large samples of different types of pupil—gifted, backward, dull, defective, and delinquent—with parallel samples of normal children to serve as control groups; and, so far as possible, we made studies of their family histories and followed them up into adult life. Since in those days conditions in rural areas differed widely from those obtaining in towns, similar investigations were started among children at village schools—in Warwickshire, the Cotswolds, the Cheviots, and elsewhere.[1]

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[1] See 'Proposal for Anthropological Statistics from Schools', *J. Anthrop. Inst.*, III, 1874, pp. 308–311; *Ann. Rep. Brit. Assoc.*, sect. H, 1877; *Nature*, May 6, 1880; 'Anthropometry in Schools', *J. Prev. Med.*, XIV, 1906, pp. 93–108.

One of Galton's intimate friends was F. W. Farrar, classical master at Marlborough College, and later at Harrow: (he was subsequently appointed Dean of Canterbury, and was already celebrated as the "ablest of the many able scholars to be found among Anglican divines" and the author of the still popular *Life of Christ*). Farrar was able to arrange that something like an anthropometric laboratory should be established at Marlborough and then at Harrow; and a number of other schools followed suit.

Towards the end of the century, as Galton's interest veered more and more towards Eugenics, Sully took over the psychological work of the Anthropometric Laboratory at University College; eventually it became part of the Education Department which he established there in close connection with University College School in 1893. In 1900 the work was transferred to Sully's new Psychological Laboratory under McDougall as Director.

[2] See O. J. R. Howarth (Secretary), *The British Association for the Advancement of Science: A Retrospect*, 1922, pp. 200f. As Howarth observes, the final report of this first inquiry (published in 1883) "is of particular note because on its work were based all the more recent standards and estimates, and its results were found to be of particular importance when the country took stock of its manpower during the war of 1914–18".

For Galton himself, however, the educational implications of his ' psychometric inquiries ' were something of a side-issue. More and more he had come to feel that the most urgent need was to arouse public interest in the other aspect of the human problem—the study of heredity. From 1870 onwards governments of both main parties had started introducing revolutionary changes in education, taxation, the health services, the reduction of poverty and crime, all calculated to effect marked improvements in the living conditions of the existing generation, but with little regard to their possible effects on the generations to come. Together with concurrent changes in industry, transport, and the applications of scientific discoveries, they had already produced marked alterations in class structure, in marriage customs, and in the birth-rate. Long ago, during his African travels and later during his pedigree-studies of genius, Galton (as we have seen) had been greatly impressed by " the frequency with which one race has supplanted others in various geographical areas ", and by the rise and decline not only of families, but of communities and of nations. As an anthropologist he was familiar with the practice of endogamy and exogamy among primitive tribes; he had noted how many of the most successful nations—the Greek, the British, the North American—appeared to have started with a long spell of outbreeding and crossbreeding, and that at a later stage many of the more influential clans, castes, sects, and social classes, had practised systematic inbreeding. Again and again in the past the conscious insistence on restrictive matrimonial customs and the deliberate introduction of restrictive marriage laws had shown that civilized man had already vaguely realized that, by controlling the mating system, he might also control his own evolution. And it was Galton's growing conviction, based on a firm belief in the Darwinian creed, that what hitherto had been done in relative ignorance ought in future to be guided by scientific research and rational planning.

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[1] The study of village communities has several special advantages: far more information is obtainable about the home conditions and the family histories of the children tested, and the striking effects of inbreeding, crossbreeding, and migration (particularly the exodus to the towns) can be more readily demonstrated when the investigation is repeated with the second generation.

The concept and the term 'eugenics' was first explicitly introduced and defined in his book on *Human Faculty*. At that date, however, there was so much to be achieved by the more obvious method of environmental improvement that both scientists and social reformers remained unconvinced or even hostile. Today the welfare state—the utopian dream of the nineteenth century philanthropist—is acclaimed as a reality; extremes of poverty and insanitation have been stamped out, and an educational ladder set up, free to all who can climb it. Yet backwardness and mental deficiency, delinquency and crime remain as frequent as before. It is time therefore that we took up once again the thread of Galton's argument where it was broken off, to see whether after all it may not offer a clue to the Daedalian maze.

Galton himself fully realized that the first step should be further research; and to this project, as he neared the end of his days, he generously devoted much of his fortune. Finally, a codicil to his will bequeathed " all the residue of [his] estate to the University of London for the establishment of a Professorship of Eugenics, with a laboratory and library attached thereto ".[1]

As this brief retrospect sufficiently shows, Galton, in the course of his long life, made contributions of outstanding importance to many widely different departments of knowledge—geography, meteorology, anthropology, criminology, medicine, statistics, and genetics: in any one of these fields, his novel ideas would have made the reputation of a first class scientist. He has been hailed as the pioneer of experimental psychology in this country; and he was the father of statistical psychology as we know it today. Nevertheless, it will, I believe, be chiefly for the impetus he gave to the study of individual differences by his ingenious psychological techniques—mental testing, rating scales, standardized questionnaires, coefficients of correlation and regression, and the application of the normal curve—that Galton will be remembered. When he took it up, individual psychology was just a speculative topic for the fancies of the poet, the novelist, the biographer, and the quack and charlatan on the seaside pier. By the time he left it and handed it on to others, it had been transformed into a reputable branch of natural science—perhaps for mankind the most important branch there is.

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[1] In 1901 the journal *Biometrika* was founded by Galton, Pearson, and Weldon, largely to publish the work of the biometric laboratory at University College; the Eugenic Record Office in Gower Street was opened in 1904; in 1906 he endowed a Research Fellowship; two years later the Eugenics Society was formed; and in 1911, the year of Galton's death, Karl Pearson was appointed to the Galton professorship. The *Annals of Eugenics* started as a natural extension of *Biometrika*, and from 1934 onwards, with Sir Ronald Fisher (Pearson's successor in the Galton chair) as editor, became the vehicle for many important papers by Fisher and his associates. Under Fisher too a serological unit was established (now transferred to the Lister Institute). Today there are Institutes for Eugenic research, and Eugenics societies or their equivalent, not only in Britain and America, but in at least half-a-dozen European countries. In Britain almost every leading geneticist—William Bateson, Sir Ronald Fisher, Sir Julian Huxley, Professor J. B. S. Haldane, Professor Darlington, Professor Mather, Professor Penrose—has made contributions to the problems of eugenics and of human inheritance. For a further account of the progress of this aspect of Galton's work, see the admirable survey by C. P. Blacker, *Eugenics: Galton and After* (Duckworth, 1952).

APPENDIX

Galton on ' The Natural Ability of Nations '

The interesting comments which Dr. Isaacs has been good enough to send (see this *Journal* p. 76 below) deserve at least a tentative reply. Like several other critics of my conference paper, he regrets that I did not devote more space to Galton as ' the apostle of eugenics '. My excuse is simply the magnitude and the obscurity of the whole subject. To his final question the short answer is that most present-day geneticists would probably endorse what is vital in Galton's views while modifying much of the detail, whereas most present-day psychologists appear to reject or ignore them almost entirely.

As I have indicated above (p. 40), the basis of Galton's eugenic theories was his firm belief that the rise and decline, not only of families, but also of social classes and nations, cannot possibly be explained solely in terms of environmental conditions—i.e. by the political, technological, economic, and cultural conditions of the time or place; they are largely, if not mainly, the effect of the two universal factors in all evolution—variation and selection. The changes themselves, however, have been spasmodic rather than progressive—a kind of ' evolutionary snakes and ladders ' (if I may borrow Mr. Aldous Huxley's phrase). Owing to the constant interaction between environment and heredity, the ' structural forms ' (as Galton calls them) both of individuals and of groups tend to get cumulatively modified until they reach a stage of relative stability (cf. *Natural Inheritance*, chap. III); the changes, however, still continue, and sooner or later the state of equilibrium breaks down. Galton cites a number of instances; but he puts them forward, not as furnishing " decisive evidence for demonstrable conclusions ", but rather as illustrations of a hypothesis that calls urgently for further research. Nowhere did he attempt to " lay down a blueprint for a future Utopia ".

We may at once admit that he was inevitably handicapped by the lack of any precise knowledge about the actual mechanism of genetic transmission. For him ' Nature ' is virtually synonymous with heredity in the narrower sense—with individual differences due to differences in *ancestry*. Today we should also recognize the influence of *mutation*—occasional modifications in the genes themselves producing inheritable differences which are usually unfavourable but sometimes beneficial. But there is a third and far more important source of individual differences of which he was wholly unaware—the segregation of genes and their chance *recombination* in sexual reproduction. These last two processes fulfil the function of what Darwin called ' spontaneous variation ', with which he coupled the theory of ' blended inheritance '.[1] Subsidiary effects, such as ' dominance ' and ' linkage ', were of course quite unknown in Galton's day. Galton himself certainly realized that what are actually transmitted must be ' potentialities or tendencies ' rather than observable characteristics; but too often he treats the two as identical. Finally, he undoubtedly overestimates the amount of innate difference between one ' race ' and another; and indeed his constant use of the term ' race ' is somewhat unfortunate.[2] But all this makes far less difference to his basic arguments than most critics apparently suppose. And his theory certainly merits a fresh examination, if only because of its relevance to our own situation, now that the ‘ ice on the rivers of international life ’ seems to be breaking up.

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[1] McDougall adopts both these Darwinian principles quite uncritically together with Buckle's rather naïve conception of the ' race-making period ' (e.g., *The Group Mind*, 1920, pp. 208f.). Let me add therefore that I should accept most of Dr. Isaacs' criticisms of ' McDougall's biology '. But the worst defect of his eugenic writings is his wholesale neglect of contemporary developments of the Mendelian theory—already so fully discussed in this connection by Bateson and the Whethams (e.g., *The Family and the Nation*, 1910). In McDougall's case this was the result of Karl Pearson's drastic criticisms of Mendelism as applied to man. Myres, I fancy, often attributes to ' replacement ' (e.g., by conquest and extermination) changes in physical characteristics that may more probably be ascribed to Mendelian ' dominance '—a process rightly emphasized by Fleure.

[2] Neither Galton nor McDougall defines the word ' race '. Both accept the late nineteenth century conception which treats human races as fairly clear-cut subspecies; both overlook the fact that almost every ' race ' known to history is of very mongrel origin and that differences in language and culture form very poor criteria of race. I shall use the term as a convenient name for what other writers (a little clumsily) have called an ' ethnic group ': like such words as ' type ', it denotes what is essentially a statistical concept.

The outstanding fact of world history has been the continuous rise of new civilizations and the decay of every civilization previous to our own. The early civilizations of the fertile river valleys—the Nile, the Euphrates, the Indus; the Minoans, the Hittites, the Mitanni; the Persians, the Greeks, the Romans; the empires of the Arabs and the Turks; the ancient cities of Mexico and Peru—all have vanished, some leaving hardly a trace. If pre-communist China seems to provide an exception, it did so largely through a process of protracted stagnation. Five thousand years ago Europe was on the same primitive level as the natives of North America in the 17th century, or the Bushmen whom Galton encountered in the corners of Damaraland, and the tribes of New Guinea at the present day. Why then did the Europeans not remain illiterate Stone Age savages like the Red Indians or the Papuans? A partial answer is that they appropriated the various cultural devices—agriculture, domestication of cattle, metallurgy, writing—from their Asiatic or Egyptian neighbours. Somewhere about the third millennium B.C. “ a variety of large-built, large-headed men, of exceptional drive and force of character ” (we are told)—the socalled ‘ prospectors ’—led in the first instance by some original genius, who combined the qualities of Noah, Columbus, and the Pirate-King, ventured to cross the seas, searching for, or trading in, copper and tin, and so began importing the Bronze Age civilization of the Nile Delta into Crete—an obvious centre for converging influences and at the same time a convenient bridge; thence they carried it to the Aegean Islands and to Cyprus (the ‘ copper isle ’), and so to the coasts of Hellas and Asia Minor.[1] After that, “ all could raise the flowers, for all had got the seed ”. Yet this alone can hardly explain how these upstart barbarians manage in the end to outstrip the civilizations of Egypt and Mesopotamia, and, before many centuries were over, vanquish them in their own home lands.

These, in modernized dress, were the kind of problems that Galton sought to answer. His solution was much the same as William James ’. “ What,” says James, “ can be the causes that make a community change from generation to generation ? I reply—the accumulated influence of individuals . . . in a word, the presence or the lack of great men.”[2]

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[1] Cf. J. L. Myres, *Who Were the Greeks?* (1930); Gordon Childe, *The Pre-history of European Society* (1958). The racial affinities of these founders of the ‘ Early Minoan ’ civilization is something of a mystery. A number of archaeologists have held that they were “ neither Indo-Europeans nor Semites, but possibly a late Neolithic people related to the Basques ”; Sir Arthur Evans identifies them with the Tehenu—refugees driven out from the western Delta, when Narmer (Menes) first united the two Egyptian kingdoms. Quite recently, however, Professor C. H. Gordon claims to have shown that the language of the pre-Hellenic Cretans—the Ετεόκρητοι of Homer—was a variety of North-West Semitic (related to Ugaritic), which in turn suggests that the ‘ Early Minoans ’ were in fact Phoenicians (‘ dark reds ’, as their Greek name implies). Homer, it will be remembered, describes Minos as the son of a princess of Tyre (Europa); and Danaos was a nephew of the King of Phoenicia and son of the King of Egypt. Thus, as so often, legend embodies a fragment of the truth. (I understand that Professor Gordon’s studies of the Cretan texts—which are mostly written in the Linear A syllabary, though the latest are in Greek letters—are to be published in the July number of the *Journal of Near Eastern Studies*.)

[2] W. James, ‘ Great Men and their Environment ’, *Atlantic Monthly*, 1880 (reprinted in *Essays in Popular Philosophy*). James, however, in his later footnotes introduces one or two criticisms of Galton’s views (for whose “ laborious investigations of the heredity of genius ”, he tells us, he has “ the greatest respect ”): they resemble those now raised by Dr. Isaacs, and, like his, seem due to taking emphatic statements out of their context. Galton, he says, argues that “ genius, like murder, ‘ will out ’ regardless of opportunity: yet surely many geniuses must have died ‘ with all their music in them ’ ”. There must in addition be an environmental need and an environmental selection. The man and the environment are both essential: the crisis must arise to provide the stimulus, and a ‘ great man ’ must be there who can rise to the occasion. “ The relation of the environment to the great man is exactly what it is to the ‘ variation ’ in the Darwinian philosophy of evolution: it adopts or rejects admires or condemns, in short—it *selects* him.” But all this is precisely Galton’s own theory.

Galton believed that he had seen the fundamental causes actually at work, on a small scale but in a typical way, during his travels in South Africa. However, in his later writings he relies chiefly on familiar illustrations drawn from European history. And most historians of the present time (though by no means all) would, I think, agree that he had made out a strong *prima facie* case in his two main instances.

(i) During the century which lasted from about 490 B.C. (battle of Marathon) to 399 B.C. (death of Socrates), Athens, with a population of under 70,000 freeborn males aged 25 or over, produced at least fourteen men of the highest intellectual eminence—a proportion of 1 in 5,000, whereas the average for other nations and Galton's own frequency curve would lead us to expect only 1 in several million.[1] During the 130 years that have elapsed since her liberation from the Turks, with a population ten times that number, she has produced not a single person anywhere near that rank. No doubt in the 5th century B.C. the political and cultural events that occurred in and around Greece just before and during the Periclean age may have acted as precipitating causes. But similar events occurred at other times and in other places without creating anything like this spectacular efflorescence of genius. And both contemporary and later historians have insisted that there must have been something quite exceptional about the character of the people at that time—a " natural disposition which gave them an almost excessive love of freedom, enterprise, and independence ".[2] How exceptional it seemed to others is shown by Xerxes' remark: " If as you say this handful of men are so fond of freedom, then surely they will run away " (*Herodotus*, VII, 104). The conclusion, says Galton, seems inescapable: " by a system of unconscious selection Greece had built up a magnificent breed of human animals—the ablest race of whom history bears a record ".

(ii) To explain the rise of nations has seemed to most writers relatively easy.[3] Galton, like Gibbon musing among the ruins of ancient Rome, found it far harder and far more urgent to account for their decline and fall. Once a civilized society has established and organized itself, how does it ever come to pass that in the course of time it eventually fails in competing for survival with one or other of its less civilized neighbours ?

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[1] *Hereditary Genius*, pp. 341f. Galton's list comprises Miltiades, Cimon, Themistocles, Aristides, Pericles, Socrates, Plato, Xenophon, Thucydides, Aeschylus, Sophocles, Euripides, Aristophanes, and Phidias. (He puts the military leaders first because without their courage and brilliant strategy Greece would have undoubtedly succumbed to Persia.) If we go beyond the walls of Athens, we could add other names belonging to the same remarkable ' racial stock '—Zeno, Protagoras, Empedocles, Democritus, Anaxagoras, Hippocrates, etc. In the text I have ventured to make slight emendations in Galton's dates and figures. Galton's own comments on his list dispose of the other criticism made both by James and by Dr. Isaacs. " Mr. Galton," says James, " inclines to think that within any given race an equal number of geniuses of each grade must be born in every equal period of time." But Galton's whole point is that, in cases like the Athens of Pericles and the England of Elizabeth I, the number was *exceptional*; and by the laws of probability exceptional numbers are just what we should from time to time expect. Every whist player knows that, although we consider ourselves lucky when a hand contains more than 2 or 3 trumps, once in about 300 deals we can expect as many as 8 or 9 together. And it is the unusual concourse of geniuses, all born at about the same time, that makes such eras famous in history.

[2] Cf. Herodotus, I, 60 : " The Greek race was of old distinguished from foreign nations as more intelligent and more emancipated; . . . and of all the Greeks the Athenians were counted first in wisdom " (see also id., V, 78 and Thucydides, II, 35–46).

[3] That is the verdict of Dr. Isaacs. " The obvious and familiar reason for the ascendancy of the ancient Greeks," he maintains, " was simply the *migration* of the Greek race into the Greek peninsula at the dawn of their history, and, as a consequence of the migration, *conquest*, followed in the natural course of things by the confident sense of peace and security that a victorious nation always enjoys and the opportunities for cultural development that such a peace always affords." The rejoinder is obvious. To undertake such an enterprise, and to adapt themselves to their new environment, the people concerned must be endowed with certain qualities of physique and character; and they must be inspired by leaders with initiative, courage, and resource. The story of the Israelites, hankering after the fleshpots of Egypt and finding their ark captured by the Philistines, is

The causes he suggests are after all very similar to those put forward by ancient historians themselves: first and foremost, the diminution in the birthrate among the abler and more energetic families and the relative increase in the birthrate among what might

typical of the vicissitudes to which other migrations have so often succumbed. To establish my chief points would require a long digression; but since Dr. Isaacs has challenged me for my facts, I must at least outline the evidence on which I rely.

My main assumption—or rather that of Galton as reinterpreted in terms of Mendelian theory—is that (in accordance with the principle of 'recombination') increased variability within any given group is nearly always the result of a mixture of breeds. No nation, up to the period with which we are concerned, had sprung from such mingled origins as the people we call the Greeks. The Greek peninsula itself was open to access from the South, East, North, and even the West. But the barriers of mountain and sea (as the Persians found to their cost) made penetration peculiarly difficult.

The foundation of the successive Minoan civilizations was only the prelude. But even at this early date we find Crete and the Aegean islands and shores already occupied, not only by the short long-headed 'Mediterranean' or 'Brown' race (coming perhaps from the African plains to the south of the Sea), but also by an assortment of hairy round-heads of the type that had its natural habitat in the continental forests and mountains (and therefore coming presumably from the North or East—most probably from Asia Minor). In 1400 B.C. (i.e. in terms of Egyptian chronology, shortly before the reigns of Ikhnaton and Tutankhamen) the palace régime of Knossos was suddenly and completely destroyed. But during the next two centuries we find frequent mention in Egyptian, Hittite, Jewish, and Greek records, of a fresh set of Aegean assailants—tall and formidable sea-raiders whose name is variously spelt PLST, PLSG, or PLSP (the Pelethi, the giant-like Philistines, the Pelasgians or 'sea-people', the sons of Pelops). About 1200 B.C. (roughly the time of the Israelite 'Judges') their concerted attacks were eventually defeated by Rameses III, the last of the great Pharaohs; and many of them were settled on the coastal plains of Palestine to which they gave their name. This is the legendary period of the voyage of the Argonauts, of Paris's abduction of Helen, and the counter-expedition of the Greeks against Troy, and the maritime wanderings of Odysseus. Finally there are further waves of fresh invaders, bearing such names as Akhiyava in Hittite records (Achaian in Greek) and "Javan from the isles afar off" in Jewish literature (the Hebrew letters are simply I, O, N, plainly the Greek 'Ionian'). These, and no doubt some of their predecessors, spoke various dialects of an Indo-European tongue. By now cheap iron had begun to supersede bronze in warfare, agriculture, and industry; and the later invaders from the North were horse-*riders* (looking like centaurs to their foes), not merely drivers of horses harnessed to chariots. In the Homeric poems the Achaean military chiefs are distinguished by their tall stature, their 'yellow' hair, their 'grey' eyes, their 'ivory-white' or 'rosy' skins: they must therefore represent Nordics from the North, or more probably (in view of their broadish heads as depicted by later sculptors) a mixed stock with a strong Nordic infusion. This is the physical type adopted for the Olympian Gods, the later heroes, the aristocrats, and the statuesque ideal. If further evidence were required, we have only to look at the coloured vases and the portrait-busts (of Socrates, Plato, and Pericles, for instance) to see what a very heterogeneous community the Athenians must have been about the time of which Galton speaks.

One or two geneticists have suggested that "the exceptional accomplishments of mongrel races like our own may have been due in part to 'heterosis'" (i.e. "the 'hybrid vigour' caused by the fact that in the heterozygotes certain characteristics are developed with greater intensity than in the recessive or pure dominants")—a phenomenon whose importance in the human race has, so Penrose believes, been unduly overlooked (cf. *Eugen. Rev.*, XLI, pp. 23f.). But with the Greeks at any rate the 'racial qualities', as Galton calls them, could be sufficiently accounted for by the cross-breeding, the recombinations, the wide variability, the progressive selection, the subsequent inbreeding and assortative mating. That the dictates of eugenics were not wholly strange to the Greeks themselves is shown by their stringent marriage-laws (notably in Sparta), the pride with which the various clans and families preserved their pedigrees, the widespread practice of exposing weakly infants, and above all by Plato's celebrated distinction between the classes of 'gold, silver, copper and iron' and his frequent appeal to the analogy of the breeding-stud (*Rep.* 415 B, 459 A).

Cut off as the Greek cities were in their island homes and mountain valleys, it would have been impossible at that date to organize a Greek empire, like the older empires of Egypt or Mesopotamia. Athens itself, even at the climax of its fortunes, was distinguished from every previous civilization by quite appropriately be called the ‘ proletariat ’[1]—each of the two features being a common and a natural sequel to any general rise in the standard of comfort; secondly, and in part an indirect consequence, “ the decline in public esteem for men of high originality and enterprise ”, leading to their ostracism, persecution, banishment, or death, and still more often to their voluntary migration.[2] In the case of Greece and Rome the inevitable outcome was, in Seeck’s phrase, “ eine Ausrottung der Besten ”.[3]

Galton’s favourite example is drawn from the closing phase of the Spanish Empire. Early in the sixteenth century Spain was already being drained of her finest and most adventurous blood by the expeditions to the New World. In the second half of that century under Philip II (“ a man of second-rate ability ”, says his biographer) the dungeons and *autos da fé* of the Inquisition destroyed many of the most brilliant and intellectual of his subjects; and those who survived usually did so by seeking shelter in some religious order and submitting to the rules of celibacy which it imposed. This enforced sterility was the factor on which Galton laid the greatest stress. Undoubtedly other causes contributed.

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being at once non-monarchical and non-sacerdotal. It was the first state to establish government by consent—in theory a democratic republic, in practice an aristocracy. And the ultimate aim of Pericles himself was to draw together all the Hellenic communities into a kind of United States of Greece, with Athens as “the leader and school of her fellows”. Perhaps his most appropriate epitaph would be the words of Landor: “ He raised and rewarded every kind of merit ” (*Pericles and Aspasia*, 1st ed., II, p. 297).

And so no doubt, as Dr. Isaacs argues, the peace, security and confidence which followed the rout of the Persian army, together with “ the economic, social, and political circumstances of the time ” were “ *necessary* conditions for the remarkable intellectual achievements that followed ”. But, as Galton would have maintained, they could not by themselves have constituted the *sufficient* conditions. There had also to be “the ability and the will of the people and above all of the people’s leaders ”.

[1] At Rome the *proletarii* formed the lowest of the citizen classes and were so called because “ their only contribution to the state was their *proles* (i.e. their numerous offspring) ”: cf. Cicero, *Rep.*, II, 22, 40. By the Julian laws of 17 B.C. and the Lex Papia Poppaea, passed a quarter of a century later, Augustus endeavoured to check the growing decadence of the patrician class by imposing penalties on bachelors, forbidding senators to marry below a certain rank, and granting substantial priviliges to those with three or more children. Nevertheless, as Tacitus observes, these and other measures proved a complete failure (*Annals*, II, 25). “ *Raraque in hoc aevo qui velit esse parens*,” was the poet’s comment (*Nux*, 15, formerly attributed to Ovid). And Horace sums it up in his concluding lines (*Carm.*, III, 6):

“ Aetas parentum pejor avis tulit
Nos nequiores, mox daturos
Progeniem vitiosiorem.”

[2] At Athens the change begins even before the close of the Peloponnesian war. Socrates was executed, Themistocles ostracized, Aristotle obliged to return to a foreign court. Demosthenes in his *Philippics* comments repeatedly on the decadent spirit of the people. During the Roman period the ablest Greeks were deported or invited to Rome. And the later inhabitants of Greece were descended not so much from the citizens as from the far more numerous body of foreign slaves. Malaria and other epidemics hastened the decline. From the sixth century onwards there was a large infiltration of Slavs. The Ottoman rule introduced a further admixture of Turks: (originally Mongolian nomads, they had acquired from the Armenoid beauties of Asia Minor whom they swept into their harems a Levantine countenance: this, as we see in the Jews, is a physiognomy that tends to show Mendelian ‘ dominance ’: the broad hairy face, with thick curved nose, which they share with the Turks, is not a ‘ Semitic ’ trait as is popularly supposed). As a result of all these changes the population of Greece is today quite different from that of classical times: it is far more brachycephalic. “ The only wonder,” says Myres (*loc. cit.*, p. 27), “ is that it is still possible to trace elements which are none of the admixtures, but apparently continuous in descent from . . . Hellenic times.” To the Mendelian, however, it is hardly a “ wonder ”, but precisely what we should expect from the constant process of gene-segregation.

[3] *Geschichte des Unterganges der Antiken Welt*: J. L. Myres, ‘ Changes in Population in the Classical World ’, *Eugen. Rev.*, IX, pp. 193f.

There was, for example, the unexpected influx of silver from the West, the equally unforeseen rise in prices as a result, and a consequent social upheaval. There were too the various events beyond the borders of Spain which ultimately brought about the alliance of France with the two chief seafaring nations—England and Holland; and, as it happened, each of the three countries at that time possessed an exceptionally shrewd and energetic leader. Before the century closed, Philip was beaten and bankrupt. But these very circumstances themselves depend in part on psychological factors. And thus, with certain minor reservations, most contemporary historians would, I think, accept Galton's account of the matter as in all probability containing a large element of truth.

In man no doubt 'mental evolution' differs profoundly from 'mental evolution' in animals, owing partly to the control he is able to exercise over his own environment and partly to the preponderant role played in his development by social conditions. Hence Galton's application of the Darwinian theory to human evolution requires some qualification. Nevertheless, since he wrote, an increasing amount of evidence has accumulated to show that the kind of qualities he had in mind—health, longevity, fertility, intelligence, energy, and stability of temperament—are all largely dependent on genetic factors. This means that genetic factors must inexorably limit what even the most favourable environmental conditions can achieve, and may themselves sometimes compensate for the lack of favourable conditions.

Moreover, although the *average* innate qualities of different 'races' differ but slightly,[1] an almost indiscernible superiority in the mean level may produce quite a large difference in the relative number at the upper end of the scale. What is still more important—the *range* of variation is itself liable to fluctuate appreciably from one community to another, and even within the same community at different times. Thus, in industrial civilizations mutant genes not only occur more frequently, they are also less frequently wiped out. Consequently populations living under such conditions show a much wider range of variation. Indeed I would venture to guess that the real handicap of the socalled primitive races has been, not their low average level, but their relative homogeneity.

From all this it follows that the mating system adopted by this or that nation or social class is quite as important as its initial genetic constitution. Frequently, it would seem, the first generation after racial or social crossing is marked by increased vigour, but this immediate effect rapidly disappears. In general, outbreeding tends to weaken adaptation to the present environment, but confers greater versatility with which to meet future environmental change; inbreeding tends to break up the total community into smaller castes or classes, and intensify their peculiarities, good as well as bad. The extremes are seen in the polygamous system of the Islamic conquerors and the rigid caste system of India. In view of the rapid changes occurring in the matrimonial customs of our own country, it is urgently desirable that studies of the apparent effects should be undertaken while the data are still available for observation and analysis.

Since the rediscovery in 1901 of Mendel's early work, both genetics and molecular biology have made astonishing strides. Far more is now known about the processes of human inheritance and the complex interactions between genetic constitution, embryological and physiological development, and conditions in the environment. Consider, for instance, the implications of one comparatively recent discovery, namely, the existence of marked differences in the frequency of different blood-types among different racial types. These happen to be quite unaffected by environmental agencies. Moreover, they appear to be correlated, not only with differences in the liability to certain diseases, but also with certain behavioural peculiarities, e.g., differences in the pronunciation of certain consonants. The correlation is so unexpected that (as Dr. Isaacs says) it needs a word or two of explanation.

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[1] The alleged inferiority of the negro races in innate general intelligence is not borne out by intelligence tests in which due allowance has been made for cultural differences. Even in the early American army tests the negroes in the Northern States did better than the whites in the Southern States. On the other hand, the black aborigines of Australia (a far more homogeneous stock than the negroes of Africa or the U.S.A.) have low I.Q.'s, and have failed almost entirely to absorb the education and culture of the European colonists—quite unlike the Maoris of the neighbouring islands of New Zealand: (one Maori even rose to cabinet rank).

It is recorded that before the Israelites were firmly established to the East of the river Jordan the ' men of Gilead ' fought with the ' men of Ephraim '; and, to distinguish friend from foe, they put to any suspicious fugitive some kind of question which would require in reply the Hebrew word for ' stream ' (*Shibboleth*). The Ephraimites pronounced it (as we should pronounce our own word) with a simple sibilant (*s*); the Gileadites, however, pronounced it (as a German might) with a ' broad ' fricative (*sh*).[1] There is an analogous difference in the pronunciation of dentals. Certain ethnic groups find it quite natural to pronounce *fricative* dentals (as in *faTHer*, *broTHer*, or *THree*); the rest substitute a *plosive* (as in *VaTer*, *BruDer*, *Drei*). Now if a map is made of this linguistic difference, the present frontier between the two coincides almost exactly with the 64·5 per cent contour-line separating Western and Southern regions of Europe in which the recessive O blood-type predominates from those in which the A and B types predominate.[2] Trifling as it may seem, this well established correlation plainly suggests that differences in blood-groups might provide serviceable ' ethnic markers ' for psychogenetic investigations. Other scientific discoveries suggest other modes of approach. A.I.D. (artificial insemination from a donor), for example, appears likely to become increasingly frequent; if so, the records—provided they are systematically compiled—should furnish useful empirical tests for various psychogenetic hypotheses. Thus it would now seem that genetic researches of the type that Galton demanded are by no means so impracticable as Dr. Isaacs and others have supposed. But at the present stage by far the most fruitful line of attack will be to carry out intensive studies of breeding systems and behavioural inheritance in animals.[3]

Among the many geneticists who have lent their support to Galton's conclusions the most eminent is Sir Ronald Fisher. More particularly he has succeeded in combining the genetic approach with the biometric, and has shown how modern statistical techniques can be adapted to check the theoretical assumptions involved. In his chapters on the eugenic problem, after examining the various causes adduced by previous writers to explain " the decay of every civilization previous to our own ", he himself concludes that " the evidence examined leaves little doubt that the most powerful selective agency in civilized man is that acting upon mental and moral qualities by way of the birthrate ".[1] Since Fisher wrote, the other two factors chiefly stressed by historians—persecution and migration—have (as Dr. Isaacs observes) also been operative on an extensive scale; and to the depletion thus resulting several writers have recently ascribed " the marked deterioration in the quality of German post-war contributions to the various sciences, as well as other more familiar results " (p. 78 below). But here I agree with him that environmental factors have also cooperated, so that " we are scarcely justified in looking in that direction for definite verification ". It is always precarious to rely on mere deductive arguments to reach solutions to biological or sociological problems. At present, however, the nearest approach to direct confirmatory evidence that I know of is the decline in tested ability among school children in certain English villages during the past fifty years, where there has been either a marked change in the differential birthrate or an exodus of the brightest teenagers to the neighbouring towns. But, when all is said, the most important conclusion to be drawn is that on which Galton always laid the greatest stress—namely, the pressing need for more extensive and carefully planned research.[2]

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[1] *Judges*, XII, 6. Here the ' ethnic implications ' about which Dr. Isaacs asks are largely guesswork. It is commonly supposed that the legend embodies recollections of Abrahamic rather than Mosaic traditions. It would seem that about the 4th millennium an 'Alpine ' stock (doubtless related to the Indo-European people who, according to the Boghaz-keui inscription, later entered from the North—the ' Hittites ' of Gen. XXIII) had descended from the high plateaux around Mount Ararat, settled first in Harran (the original home of Abraham and his father), then worked their way towards ' Ur of the Chaldees ', and later into Canaan, having meanwhile adopted a mispronounced Semitic speech. In that case the Habiru mentioned with the Hittites in the El Amarna tablets (c. 1380 B.C.) would be a mixed Armenoid stock. It would seem that they were later challenged by nomadic invaders (of a type related to the modern Bedouin) whose native tongue was Semitic and who had come up from the Arabian desert in the South (some perhaps from Egypt after the expulsion of the Semitic ' Shepherd-Kings '). This would explain the difference in pronunciation by an ethnic difference (cf. J. Garstang, *Joshua and Judges*). The attempts of the Greek (LXX) and Latin (Vulgate) translations of the verse cited above to illustrate the difference further exemplify the point, for neither language possesses the sound *sh*. A related difference, also associated with a difficulty in the pronunciation of sibilants, is seen in the familiar division of the Indo-European languages into the ' centum ' and the ' satem ' branches; originally these two seem also to have been correlated with a broad ethnic difference. A similar test was adopted during the massacre of the Sicilian Vespers (31 March, 1282); the French were made to betray themselves by their sibilant pronunciation of *ceci e ciceri*.

Julian Huxley deplores the fact that " the implications of the new knowledge (supplied by post-Mendelian genetics) have not been incorporated in anthropological writings " (*We Europeans*, p. 63); and my reason for elaborating the foregoing argument is not merely to reply to Dr. Isaacs' inquiries (p. 77) about ' ethnic implications ', but also to suggest that a Galtonian study of the evolution of the Jewish peoples would prove even more illuminating than the study of the Greeks.

[2] Cf. C. D. Darlington, ' The Genetic Component of Language ', *Heredity*, I pp. 269–286.

[3] See J. L. Fuller and W. R. Thompson, *Behaviour Genetics*, 1960.

I may sum up my own provisional conclusions as follows. Great changes in national character and culture may unquestionably occur, without any corresponding change in the innate mentality of the people as a whole, purely as a result of contact with other nations or of internal social or intellectual changes. So far as the rise and fall of nations is the result of genetic influences, they are due, not so much to any racial change, but rather to changes in the relative fertility of its leading members or its leading classes; and such changes can only operate favourably when there is already a wide range of individual variation on which the selective processes can work. Thus neither heredity alone nor environment alone but the interaction of the two forms the really effective agency.

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[1] R. A. Fisher, *The Genetical Theory of Natural Selection*, 1930, p. 209. Fisher places special weight on the fact that an able and energetic individual, who is also infertile or at any rate refrains from marriage and the production of children, is far freer to work his way up from a lower socio-economic class to a higher. As Bacon long ago observed, " he that hath wife and children hath given hostages to fortune, for they are impediments to great enterprises either of virtue or mischief; certainly, the best works have proceeded from unmarried or childless men, which have married and endowed the public " (*Essay* VIII).

Galton's illustration remains the most striking. In the past able and ambitious individuals have often sought to hasten their own advancement by marrying wealthy heiresses; peers or their sons did the same, frequently in the hope of restoring the family fortunes. But a daughter only becomes an heiress because her parents have no surviving sons. And this infertility is apparently inheritable. After examining a long list of peers, some newly created, others of ancient date, Galton found that, when the line became extinct, this had very commonly followed on a marriage with an heiress. Taking 100 heiresses who were wives of peers and comparing them with 100 peeresses taken at random, he found that whereas the latter produced on an average 3·4 sons, the former produced less than 2·1. (As we have seen, one of the stocks from which Galton himself was descended was apt to die out in the male line and Galton himself was childless: Pearson infers that it was this which directed Galton's attention to the whole subject of infertility.)

[2] These inadequate and somewhat disjointed comments are intended merely to answer the criticisms urged by Dr. Isaacs and others against Galton's general position, largely, so it seems to me, as a result of misunderstanding. They are not put forward to support any eugenic theory of my own. Those who wish for a more systematic review of the problems may refer to F. Osborn, *Preface to Eugenics*, 1951, and T. Dobzhansky, ' The Biological Concept of Heredity as Applied to Man ', ap. *Nature and Transmission of the Genetic and Cultural Characteristics of Human Population*, Milbank Memorial Fund, 1957. For the views of psychologists and geneticists regarding the relation between intelligence and fertility, see the *Memoranda presented to the Royal Commission on Population*, H.M. Stationery Office, 1950. For the views of anthropologists and historians see G. V. de Lepouge, *Race et milieu social* (1909); E. Pittard, *Les races et l'histoire* (1924); C. S. Coon, *The Races of Europe* (1939); A. J. Toynbee, *The Study of History* (1939).

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A NOTE ON SIMULTANEOUS EQUATIONS AND MATRIX INVERSION

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The following note discusses the special advantages of the 'hierarchical method' of solving linear equations, and describes and illustrates in detail the working procedure recommended for calculating inverses in the case of both symmetric and non-symmetric matrices.

For occasional and not-too-experienced computers the hierarchical form of the single-division method for the computations of linear algebra has advantages that are too often overlooked.[1] Writers on linear computation are usually themselves expert computers; and hence favour either the compact square root method or the abbreviated Doolittle method, or (less frequently) the compact form of the single division method (the Crout method). As compared with these the hierarchical method possesses the following advantages:

(1) Provided a desk calculator having locked multipliers is used, there is less actual punching of keys for the main routine than with any other method except the compact square root method carried out with an automatic square-root machine. This largely offsets the disadvantages due to the fuller recording of the intermediate results needed by the hierarchical procedure.

(2) A sum-check is used with every row, so that far less re-computing is necessary when a mistake has been made. The additional computations thus entailed, however, outweigh the advantage of locked multipliers, so that, on the whole, the hierarchical method requires rather more computing and considerably more recording than either the abbreviated or the compact methods. Experienced computers should therefore use the latter; but those who are liable to make mistakes will finish faster if they use the fuller hierarchical procedure.

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[1] The 'hierarchical method' has been described by Burt [1], pp. 36–38, 41–46, and Appendix on 'Working Procedure', pp. 50–54. As he observes (pp. 32f. 'Historical Note') the underlying principle goes back to Gauss's method for 'combing observations' by the method of least squares [4]. See also Dwyer [2], pp. 99–103, 148–150, 190–203, and Fadeeva [3], pp. 65–70, 72–75, 85–88.

Editor's Note.—As this use of the term 'hierarchy' may not be familiar to some of our readers, it is perhaps necessary to explain that the word was introduced by British factorists to mean 'a matrix of rank one'. Apart from the convenient substitution of one word for four, this usage has the further advantage that it provides an adjective. Burt's original name for the procedure was 'the method of hierarchical subtraction'. In his illustrative example (intended for beginners) the 'hierarchical' set of values to be subtracted are written out explicitly, so that in the 'forward solution' there are virtually *twice* as many panels as there are variables. After a little experience, the subtraction can be carried out on the machine as described by Dr. Cureton below (pp. 52–3).

(3) The hierarchical method is the easiest of all to derive. Much more algebra is required to explain to those who use an abbreviated or compact method exactly what they are doing.

(4) The hierarchical forward solution requires only four machine operations: division to find a reciprocal, multiplication across a row by a locked reciprocal,

TABLE I. BASIC LAYOUT

	(1)	(2)	(3)	(4)	(0)	(Σ)	
(1)	$[a_{11}]$	a_{12}	a_{13}	a_{14}	$-a_{10}$	$a_{1\Sigma}$	
(2)	a_{21}	a_{22}	a_{23}	a_{24}	$-a_{20}$	$a_{2\Sigma}$	
(3)	a_{31}	a_{32}	a_{33}	a_{34}	$-a_{30}$	$a_{3\Sigma}$	
(4)	a_{41}	a_{42}	a_{43}	a_{44}	$-a_{40}$	$a_{4\Sigma}$	
(u_1)	-1	u_{12}	u_{13}	u_{14}	u_{10}	$u_{1\Sigma}$	
(2)		$[a_{22\,.\,1}]$	$a_{23\,.\,1}$	$a_{24\,.\,1}$	$a_{20\,.\,1}$	$a_{2\Sigma\,.\,1}$	
(3)		$a_{32\,.\,1}$	$a_{33\,.\,1}$	$a_{34\,.\,1}$	$a_{30\,.\,1}$	$a_{3\Sigma\,.\,1}$	
(4)		$a_{42\,.\,1}$	$a_{43\,.\,1}$	$a_{44\,.\,1}$	$a_{40\,.\,1}$	$a_{4\Sigma\,.\,1}$	
(u_2)		-1	u_{23}	u_{24}	u_{20}	$u_{2\Sigma}$	
(3)			$[a_{33\,.\,2}]$	$a_{34\,.\,2}$	$a_{30\,.\,2}$	$a_{3\Sigma\,.\,2}$	
(4)			$a_{43\,.\,2}$	$a_{44\,.\,2}$	$a_{40\,.\,2}$	$a_{4\Sigma\,.\,2}$	
(u_3)			-1	u_{34}	u_{30}	$u_{3\Sigma}$	
(4)				$[a_{44\,.\,3}]$	$a_{40\,.\,3}$	$a_{4\Sigma\,.\,3}$	
(u_4)				-1	u_{40}	$u_{4\Sigma}$	
(R)	R_1	R_2	R_3	R_4		r^2_{m1}	Symmetric Case Only
(B)	B_1	B_2	B_3	B_4		r^2_{m2}	
(a')	a'_1	a'_2	a'_3	a'_4		$\lvert A\rvert$	

	(1)	(2)	(3)	(4)	(1)	(2)	(3)
(1)	A_{11}	A_{12}	A_{13}	A_{14}	(g_{11})		
(2)	A_{21}	A_{22}	A_{23}	A_{24}	g_{21}	(g_{22})	
(3)	A_{31}	A_{32}	A_{33}	A_{34}	g_{31}	g_{32}	(g_{33})
(4)	A_{41}	A_{42}	A_{43}	A_{44}	g_{41}	g_{42}	g_{43}
(1)	(g_{11})	g_{12}	g_{13}	g_{14}			
(2)		(g_{22})	g_{23}	g_{24}			
(3)			(g_{33})	g_{34}			
(O)	a_{10}	a_{20}	a_{30}	a_{40}			
(B)	B_1	B_2	B_3	B_4			

Computation of inverse (non-symmetric case) and solution of simultaneous equations from inverse.

	(1)	(2)	(3)	(4)
(1)	I_{11}	I_{12}	I_{13}	I_{14}
(2)	I_{21}	I_{22}	I_{23}	I_{24}
(3)	I_{31}	I_{32}	I_{33}	I_{34}
(4)	I_{41}	I_{42}	I_{43}	I_{44}

Substitution check for inverse (reproduced identity matrix).

the operation $a+bc$ with b locked, and addition (including subtraction) across a row to obtain a check. The abbreviated and compact methods use much more complicated machine operations, e.g., $(a-bc-de-fg)/h$.

(5) The hierarchical method virtually eliminates 'finding' or 'reading' mistakes. In the $a+bc$ operation, b is a locked multiplier, and a and c are both in the same column as the cell in which the result is to be entered.

(6) The method itself is so simple and repetitive that it can be learned by working only *one or two* examples. Thereafter it is not necessary to refer to computing instructions or lists of equations except in the back solution for an inverse.

TABLE II. EQUATIONS : FOUR-VARIABLE CASE

A. *Forward Solution:*

$$\begin{aligned} R_1 &= 1/a_{11} \\ u_{1j} &= -R_1 a_{1j} \\ a_{ij.1} &= a_{ij} - a_{i1} u_{1j} \\ R_2 &= 1/a_{22.1} \\ u_{2j} &= -R_2 a_{2j.1} \\ a_{ij.2} &= a_{ij.1} - a_{i2.1} u_{2j} \\ R_3 &= 1/a_{33.2} \\ u_{3j} &= -R_3 a_{3j.2} \\ a_{ij.3} &= a_{ij.2} - a_{i3.2} u_{3j} \\ R_4 &= 1/a_{44.3} \\ u_{4j} &= -R_4 a_{4j.3} \end{aligned}$$

B. *Back Solution : Simultaneous Equations*

$$\begin{aligned} &B_4 = u_{40} \\ &B_3 = u_{30} + u_{34} B_4 \\ &B_2 = u_{20} + u_{24} B_4 + u_{23} B_3 \\ &B_1 = u_{10} + u_{14} B_4 + u_{13} B_3 + u_{12} B_2 \end{aligned}$$

C. *Check : Symmetric Case Only*

$$\begin{aligned} &r^2_{m1} = -a_{10} u_{10} - a_{20.1} u_{20} - a_{30.2} u_{30} - a_{40.3} u_{40} \\ &r^2_{m2} = a_{10} B_1 + a_{20} B_2 + a_{30} B_3 + a_{40} B_4 \\ &r^2_{m1} \doteq r^2_{m2} \end{aligned}$$

D. *Substitution Check : General Case*

$$\begin{aligned} &a_{11} B_1 + a_{12} B_2 + a_{13} B_3 + a_{14} B_4 = a'_1 \doteq a_{10} \\ &a_{21} B_1 + a_{22} B_2 + a_{23} B_3 + a_{24} B_4 = a'_2 \doteq a_{20} \\ &a_{31} B_1 + a_{32} B_2 + a_{33} B_3 + a_{34} B_4 = a'_3 \doteq a_{30} \\ &a_{41} B_1 + a_{42} B_2 + a_{43} B_3 + a_{44} B_4 = a'_4 \doteq a_{40} \end{aligned}$$

E. *Determinant*

$$|A| = a_{11} a_{22.1} a_{33.2} a_{44.3}$$

F. *Back Solution : Inverse : Symmetric Case*

$$\begin{aligned} &A_{44} = R_4 \\ &A_{43} = u_{34} A_{44} \\ &A_{42} = u_{24} A_{44} + u_{23} A_{43} \\ &A_{41} = u_{14} A_{44} + u_{13} A_{43} + u_{12} A_{42} \\ &A_{14} = A_{41}, \quad A_{24} = A_{42}, \quad A_{34} = A_{43} \end{aligned}$$

$$\begin{aligned} &A_{33} = u_{34} A_{34} + R_3 \\ &A_{32} = u_{24} A_{34} + u_{23} A_{33} \\ &A_{31} = u_{14} A_{34} + u_{13} A_{33} + u_{12} A_{32} \\ &A_{13} = A_{31}, \quad A_{23} = A_{32} \end{aligned}$$

$$\begin{aligned}
A_{22}&=u_{24}A_{24}+u_{23}A_{23}+R_2\\
A_{21}&=u_{14}A_{24}+u_{13}A_{23}+u_{12}A_{22}\\
A_{12}&=A_{21}\\
A_{11}&=u_{14}A_{14}+u_{13}A_{13}+u_{12}A_{12}+R_1
\end{aligned}$$

G. *Back Solution : Inverse : Non-Symmetric Case*

I.

$$\begin{aligned}
A_{44}&=g_{44}\\
A_{34}&=g_{43}A_{44}\\
A_{24}&=g_{42}A_{44}+g_{32}A_{34}\\
A_{14}&=g_{41}A_{44}+g_{31}A_{34}+g_{21}A_{24}
\end{aligned}$$

II.

$$\begin{aligned}
A_{43}&=(g_{34}A_{44})\,g_{33}\\
A_{42}&=(g_{24}A_{44}+g_{23}A_{43})\,g_{22}\\
A_{41}&=(g_{14}A_{44}+g_{13}A_{43}+g_{12}A_{42})\,g_{11}
\end{aligned}$$

III.

$$\begin{aligned}
A_{33}&=g_{43}A_{43}+g_{33}\\
A_{23}&=g_{42}A_{43}+g_{32}A_{33}\\
A_{13}&=g_{41}A_{43}+g_{31}A_{33}+g_{21}A_{23}
\end{aligned}$$

IV.

$$\begin{aligned}
A_{32}&=(g_{24}A_{34}+g_{23}A_{33})\,g_{22}\\
A_{31}&=(g_{14}A_{34}+g_{13}A_{33}+g_{12}A_{32})\,g_{11}
\end{aligned}$$

V.

$$\begin{aligned}
A_{22}&=g_{42}A_{42}+g_{32}A_{32}+g_{22}\\
A_{12}&=g_{41}A_{42}+g_{31}A_{32}+g_{21}A_{22}
\end{aligned}$$

VI.

$$A_{21}=(g_{14}A_{24}+g_{13}A_{23}+g_{12}A_{22})\,g_{11}$$

VII.

$$A_{11}=g_{41}A_{41}+g_{31}A_{31}+g_{21}A_{21}+g_{11}$$

$$\begin{aligned}
g_{ii}&=R_i\\
g_{ij}&=u_{ji}\ ;\ i>j \text{ in } g_{ij}{}^{*}\\
g_{ij}&=-a_{ji\,.\,(i-1)}\ ;\ i<j \text{ in } g_{ij}{}^{**}
\end{aligned}$$

H. *Substitution Check for Inverse* $(AA^{-1}=I)$

$$\begin{aligned}
a_{11}A_{11}+a_{12}A_{21}+a_{13}A_{31}+a_{14}A_{41}&\doteq 1\\
a_{21}A_{11}+a_{22}A_{21}+a_{23}A_{31}+a_{24}A_{41}&\doteq 0\\
a_{31}A_{11}+a_{32}A_{21}+a_{33}A_{31}+a_{34}A_{41}&\doteq 0\\
a_{41}A_{11}+a_{42}A_{21}+a_{43}A_{31}+a_{44}A_{41}&\doteq 0
\end{aligned}$$

$$\begin{aligned}
a_{11}A_{12}+a_{12}A_{22}+a_{13}A_{32}+a_{14}A_{42}&\doteq 0\\
a_{21}A_{12}+a_{22}A_{22}+a_{23}A_{32}+a_{24}A_{42}&\doteq 1\\
a_{31}A_{12}+a_{32}A_{22}+a_{33}A_{32}+a_{34}A_{42}&\doteq 0\\
a_{41}A_{12}+a_{42}A_{22}+a_{43}A_{32}+a_{44}A_{42}&\doteq 0
\end{aligned}$$

$$\begin{aligned}
a_{11}A_{13}+a_{12}A_{23}+a_{13}A_{33}+a_{14}A_{43}&\doteq 0\\
a_{21}A_{13}+a_{22}A_{23}+a_{23}A_{33}+a_{24}A_{43}&\doteq 0\\
a_{31}A_{13}+a_{32}A_{23}+a_{33}A_{33}+a_{34}A_{43}&\doteq 1\\
a_{41}A_{13}+a_{42}A_{23}+a_{43}A_{33}+a_{44}A_{43}&\doteq 0
\end{aligned}$$

$$\begin{aligned}
a_{11}A_{14}+a_{12}A_{24}+a_{13}A_{34}+a_{14}A_{44}&\doteq 0\\
a_{21}A_{14}+a_{22}A_{24}+a_{23}A_{34}+a_{24}A_{44}&\doteq 0\\
a_{31}A_{14}+a_{32}A_{24}+a_{33}A_{34}+a_{34}A_{44}&\doteq 0\\
a_{41}A_{14}+a_{42}A_{24}+a_{43}A_{34}+a_{44}A_{44}&\doteq 1
\end{aligned}$$

I. *Solution of Simultaneous Equations from Inverse*

$$\begin{aligned}
B_1&=a_{10}A_{11}+a_{20}A_{12}+a_{30}A_{13}+a_{40}A_{14}\\
B_2&=a_{10}A_{21}+a_{20}A_{22}+a_{30}A_{23}+a_{40}A_{24}\\
B_3&=a_{10}A_{31}+a_{20}A_{32}+a_{30}A_{33}+a_{40}A_{34}\\
B_4&=a_{10}A_{41}+a_{20}A_{42}+a_{30}A_{43}+a_{40}A_{44}
\end{aligned}$$

---

[*] Bordering *columns* at right are *u-rows* of forward solution, less -1's.

[**] Bordering *rows* at bottom are initial *columns* of panels of forward solution, less pivotal elements, with all signs reversed

(7) The forward solution is essentially the same in the non-symmetric case as in the symmetric: the only difference is that in the latter, results are copied from rows into columns as each row is completed and checked. The back solution for simultaneous equations is identical in the symmetric and non-symmetric cases, but for the inverse it is more complicated in the non-symmetric case. This is true, no matter what type of forward solution is used.

The one disadvantage of the method, aside from the slightly greater computing time when there are no mistakes, comes from the fact that each intermediate result is rounded as it is recorded, so that more rounding error is accumulated into the final results. Experience indicates, however, that this is not generally serious; only in rare cases will it be enough to affect the final results by more than two or three in the last decimal place for matrices of order up to six or eight.

The system outlined in Table I and illustrated for the symmetric case in Table III uses two modifications of the usual scheme. The first, which is quite common, consists in multiplying each pivot row by the *negative* reciprocal of the pivotal element to form each unit row. This changes the basic operation from $a-bc$ to $a+bc$. When one, two, or all three of the elements a, b, and c may be negative, algebraic addition produces fewer mistakes than algebraic subtraction of the product. The second modification is somewhat less common, but still far from original. It consists in entering the original constants with *reversed signs*. This has the effect of transposing the constants, so that each row of the forward solution, *including* the constant, represents an expression which is equal to 0. The result is that the equations of the back solution consist entirely of algebraic *sums* of products, thus reducing once again the number of sign mistakes.

In Tables I, III and IV there are as many *panels* (separated by horizontal lines) as there are variables (here four). In the first panel we have the original coefficients and constants. In each succeeding panel, the row and column corresponding to the first row and column of the preceding panel are dropped, until the last panel contains only the pivot row and the u-row. The pivotal elements are emphasized by enclosing them in brackets, and their reciprocals are recorded in row (R) just below the last panel. To minimize accumulated rounding errors each reciprocal should be entered to at least *one more significant figure* than the number of decimals in the other entries, and so used in computing the values for the next unit row.

With the usual 10-bank calculator, exceptional computing simplicity is attained by working uniformly at four decimals. Decimal markers are first set at four and eight decimals in the keyboard and the product dial. For the $a+bc$ operation, b is first locked as a multipliers at four decimals. Then a is set at eight decimals, and transferred by addition or subtraction to the product dial. Finally, c is set at four decimals, bc is added algebraically to a by cumulative or negative multiplication; and the result is rounded and recorded to four decimals. There are still two keyboard columns for integral digits of a.

TABLE III. ILLUSTRATION : SYMMETRIC CASE

	(1)	(2)	(3)	(4)	(0)	(Σ)
(1)	[1]	0·4703	0·4228	0·6343	−0·3286	2·1988
(2)	0·4703	1	0·6078	0·4527	−0·3726	2·1582
(3)	0·4228	0·6078	1	0·4297	−0·4307	2·0296
(4)	0·6343	0·4527	0·4297	1	−0·3333	2·1834
(u_1)	−1	−0·4703	−0·4228	−0·6343	0·3286	−2·1988
(2)		[0·7788]	0·4090	0·1544	−0·2181	1·1241
(3)		0·4090	0·8212	0·1615	−0·2918	1·0999
(4)		0·1544	0·1615	0·5977	−0·1249	0·7887
(u_2)		−1	−0·5252	−0·1982	0·2800	−1·4434
(3)			[0·6064]	0·0804	−0·1773	0·5095
(4)			0·0804	0·5671	−0·0817	0·5658
(u_3)			−1	−0·1326	0·2924	−0·8402
(4)				[0·5564]	−0·0582	0·4982
(u_4)				−1	0·1046	−0·8954
(R)	1·0000	1·2840	1·6491	1·7973		
(B)	0·0914	0·1130	0·2785	0·1046	$r^2_{m1}=0·2270$	
(a')	0·3286	0·3726	0·4308	0·3334	$r^2_{m2}=0·2270$	
					$\|A\| =0·2628$	

Inverse

	(1)	(2)	(3)	(4)
(1)	1·8168	−0·3319	−0·1665	−0·9306
(2)	−0·3319	1·7686	−0·8355	−0·2311
(3)	−0·1665	−0·8355	1·6807	−0·2383
(4)	−0·9306	−0·2311	−0·2383	1·7973
(0)	0·3286	0·3726	0·4307	0·3333
(B)	0·0915	0·1130	0·2784	0·1045

Reproduced Identity Matrix

	(1)	(2)	(3)	(4)
(1)	1·0000	0·0001	0·0000	0·0000
(2)	0·0001	1·0001	−0·0002	0·0000
(3)	0·0000	−0·0002	1·0001	0·0001
(4)	0·0000	0·0000	0·0001	1·0000

As each row is completed through column (0), its algebraic sum is obtained and entered in column (Σ). The last $a+bc$ row-operation, using the a and c values from column (Σ), is then performed. The agreement should be close—within two or three figures in the last decimal. If this is the case, a check mark is placed after the actual row-sum. It is necessary to use the actual row-sums in the succeeding computations; otherwise discrepancies would be accumulated, and we should not get good individual row-checks for the later rows. The equations of this system are given in full, for the four-variable case, in Table II.

TABLE IV. ILLUSTRATION : NON-SYMMETRIC CASE

	(1)	(2)	(3)	(4)	(0)	(Σ)
(1)	[0·3200]	0·2900	−0·3500	0·2800	0·6800	1·2200
(2)	0·1900	0·4500	−0·1400	−0·0800	−0·5700	−0·1500
(3)	−0·1200	0·1600	0·2700	0·1300	−0·4700	−0·0300
(4)	0·2600	−0·1000	0·1500	0·3200	−0·2300	0·4000
(u_1)	−1	−0·9062	1·0938	−0·8750	−2·1250	−3·8124
(2)		[0·2778]	0·0678	−0·2462	−0·9738	−0·8744
(3)		0·2687	0·1387	0·2350	−0·2150	0·4274
(4)		−0·3356	0·4344	0·0925	−0·7825	−0·5912
(u_2)		−1	−0·2441	0·8862	3·5054	3·1475
(3)			[0·0731]	0·4731	0·7269	1·2731
(4)			0·5163	−0·2049	−1·9589	−1·6475
(u_3)			−1	−6·4720	−9·9439	−17·4159
(4)				[−3·5464]	−7·0929	−10·6393
(u_4)				−1	−2·0001	−3·0001
(*R*)	3·1250	3·5997	13·6799	−0·28198	$\lvert A\rvert=0{\cdot}0230$	
(*B*)	2·0007	1·0004	3·0007	−2·0001		
(*a*′)	−0·6799	0·5702	0·4702	0·2302		

Inverse

	(1)	(2)	(3)	(4)	(1)	(3)	(3)
(1)	−1·5206	2·4374	−2·3039	2·8732	(3·1250)		
(2)	0·4191	1·2388	1·5723	−0·6954	−0·9062	(3·5997)	
(3)	−2·0424	1·4411	0·7896	1·8251	1·0938	−0·2441	(13·6799)
(4)	2·3221	−2·2671	1·9917	−0·2820	−0·8750	0·8862	−6·4720
(1)	(3·1250)	−0·1900	0·1200	−0·2600			
(2)		(3·5997)	−0·2687	0·3356			
(3)			(13·6799)	−0·5163			
(0)	−0·6800	0·5700	0·4700	0·2300			
(*B*)	2·0013	1·0002	3·0011	−2·0000			

	(1)	(2)	(3)	(4)
(1)	1·0000	0·0000	0·0000	0·0000
(2)	−0·0002	1·0002	−0·0001	0·0000
(3)	0·0000	0·0001	1·0001	0·0001
(4)	−0·0006	0·0005	−0·0005	1·0001

Reproduced Identity Matrix

In the panels of Table I below the first, the third subscript of each *a*-entry indicates the *last* variable eliminated. In the more common notation, $a_{ij.2} = a_{ij.12}$, $a_{ij.3} = a_{ij.123}$, etc. When the original data are correlation coefficients, as in Table III, r^2_{m1} and r^2_{m2} give the square of the multiple correlation coefficient, and the B's are the standard regression coefficients. If the multiple correlation coefficient alone is desired, r^2_{m1} can be computed from the forward solution alone. Partial correlations not involving the criterion variable can be computed from the formula,

$$r_{ij.12\cdots(i-1)} = \frac{a_{ij.(i-1)}}{\sqrt{\{a_{ii.(i-1)}\,a_{jj.(i-1)}\}}}, \qquad i<j.$$

The variables eliminated are those whose indices are smaller than the smaller index for the two variables whose partial correlation is desired. The value of any determinant can be obtained from a forward solution without a column of constants; it is simply the product of all the pivotal elements.

For correlation and correlation-type data (i.e., original matrices having unities in the diagonal and numerically smaller entries elsewhere) the numerical value of the determinant identifies an almost singular case. In this case the number of trustworthy decimals in the final results (the B's, r^2_{m1} and r^2_{m2}, the elements of the inverse, etc.) is not greater than the number of decimals retained in the computations *less the number of* 0's *between the decimal point and the first significant figure of* $|A|$. The substitution checks and the check, $r^2_{m1}=r^2_{m2}$, indicate roughly the amount of accumulated rounding error in the final results; but they do *not* guard against the partial indeterminacy resulting from an almost singular matrix. A further partial check for the almost singular case, when solving simultaneous equations, is given by the rule: the number of distrustworthy decimals in the final results is not greater than the greatest discrepancy between a result from the direct back solution and the corresponding result from the inverse.

In the symmetric case, the inverse can be computed directly from the forward solution, and recorded directly below the latter, as in Table III. The multiple back solution is still considerably shorter than the reduction of the identity matrix in the forward solution and the computation of the inverse from the resulting triangular matrix. Even in the non-symmetric case, reduction of the identity matrix is not recommended. It is, however, advisable to rearrange those elements of the forward solution which are actually used in the back solution into two triangular matrices bordering the space to be used in recording the inverse. By this device the elements of the product—sums appear always in parallel positions in pairs of columns or rows. The equations must be solved in the order of their numbered groups (in Table II). Table IV gives an example of the computations for the non-symmetric case.

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THE FACTORIAL STRUCTURE OF APTITUDE TESTS AT SUCCESSIVE GRADE LEVELS[1]

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The purpose of the following research was to verify the alleged differentiation in mental abilities reported by earlier factorists. Correlations for eight tests applied to boys and girls in grades 8 to 12 have been factorized by the method of principal axes and then rotated by the varimax method. Coefficients of similarity have also been calculated for the several groups. In general, the hypothesis of differentiation is confirmed; but in certain minor respects the results obtained seem to diverge appreciably from the conclusions reached by previous investigators.

I. PROBLEMS

The following study describes the results of factorizing a series of correlation tables obtained during the standardization of the Differential Aptitude Test (Form A).[2] The object of the analysis was to investigate changes in mental organization among boys and girls by comparing the factorial structure at five successive stages of high school education. The grade in which a pupil is enrolled is of course highly correlated with his chronological age; the coefficients reported range from 0·80 to 0·85. Hence it may be assumed that the results obtained will throw light on the patterns of mental abilities at different ages during the period covered. Doppelt's (1950) factorial study of differential aptitude tests had a similar aim; but the present investigation approaches the problem from a different standpoint. Doppelt was concerned to inquire " whether a general ability factor changes as age increases, and, if so, in what direction " (Doppelt 1950, p. 21); the purpose of the present analysis was to extract as many common factors as possible, and identify them at each educational level from grades eight to twelve for both sexes. At the same time an attempt was made to determine whether the factorial structure shows systematic changes with increasing educational maturity.

Thus the investigation aimed at studying the following questions: (i) With the method of ' principal factors ' (termed by Burt and other British writers the method of ' weighted summation '), can the first of the factors so obtained be identified with ' general ability ', and, if so, is this factor the same for all groups? (ii) Does this general factor account for a decreasing amount of common variance as the educational level increases? (iii) With the method of orthogonal rotation, how far will the factors obtained at one level correspond with factors obtained at another? If they are the same, do the factor loadings show any discernible monotonic trend as a result of educational maturity? (iv) Do the results show that the differentiation among abilities established, or at least postulated, by tests of differential aptitudes becomes increasingly clear with increasing educational experience? (v) Does any particular test become more strongly saturated with any particular factor? (vi) Is there any evidence of sex differences in regard to factorial structure, and if so, do the differences become more or less marked with increase in educational experience?

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II. METHODS

Subjects. The data analysed were obtained from pupils in grades 8 to 12 in the standardization samples reported in the *DAT Manual* (Bennet, Seashore and Wesman 1952). The numbers in the groups varied from 159 to 227 for boys and from 179 to 259 for girls.

Analysis. The ten matrices of intercorrelations of the *DAT* given in the first edition of the *Manual* (1947) for boys and girls were analysed one by one by means of the principal factor method (Burt 1941, Appendix I; Harman 1960), first using the squared multiple correlations as the communality estimates, and then refactorizing the matrix with the obtained communalities. The computations were carried out with an electronic digital computer (the Royal McBee LGP 30), and a programme prepared by Johnson (1960). Except for three cases, the following criteria suggested that three principal factors are sufficient to account for the non-random common variance: (1) the contribution of the fourth principal factor was in every case less than 5 per cent; (2) successive rotations with an increasing number of principal axes satisfying the varimax criterion (Kaiser 1958) indicated that the inclusion of a fourth factor would not yield any new rotated factor, the loadings of which could be regarded as differing significantly from zero. In three cases, however, namely, grades 11 and 12 for boys and grade 12 for girls, the same criteria indicated that a fourth factor could be considered significant.

With an electronic computer the significant principal factors were then rotated to an orthogonal simple structure by using Kaiser's (1958) varimax criterion as programmed by Johnson (1960). The factors so obtained were interpreted in terms of the tests exhibiting loadings of 0·50 or more.

III. RESULTS

Factor Loadings. Tables 1 and II show the projections of all tests on the first four principal factors for the different grades, boys and girls being kept separate: h_0^2 indicates the final communality values; h_i^2 the communalities obtained after the initial factorization; and *SMC* are the squared multiple correlations of each test with the remaining $(n-1)$ tests; *TS* denotes 'Test Specificity', obtained by subtracting the final observed communality (h_0^2) from the reliability as reported in the *DAT Manual* (1952). The entries in the bottom row show the percentage of the common factor variance (i.e. of the communality) attributable to each of the four principal factors. The first factor has positive saturations for every test, and is thus a 'general' factor. It will be seen that it accounts for as much as 75 to 85 per cent of the total variance contributed by all the common factors here extracted.

Tables III and IV give the rotated varimax factor loadings. To facilitate comparison between the different groups, loadings between 0·30 and 0·50 are followed by an asterisk (*) and loadings above 0·50 by a double asterisk. The bottom row shows the percentage of common variance contributed by the different factors at different grades. Table V shows the 'Coefficients of Similarity' calculated by Burt's formula (Burt, 1941, Banks and Burt, 1954). The coefficient is essentially an 'unadjusted' correlation between factor saturations of two groups, and indicates how far a factor obtained in one group is the same as that obtained with another. The figures in the table express the degree of similarity between the different grades in regard to the first principal factor.

Interpretation of Factors. In view of values given in Tables III and IV it seems evident that Factor A should be interpreted as a general capacity for Reasoning. High factor saturations were found for Space Relations, Abstract Reasoning, Mechanical Reasoning, and Verbal Reasoning tests; very low loadings appear for Clerical Speed and Language (spelling) tests. This holds good for both boys and girls in every grade.

Factor B shows high positive loadings in Sentence Usage, Spelling, and Verbal Reasoning tests. It may therefore be identified as a Verbal factor.

The largest loading for Factor C is shown by the test of Numerical Ability. But this test also has high loadings on the two preceding factors. The Clerical Aptitude test has high factor saturation for this factor alone; and the Abstract Reasoning test exhibits a gradual increase in its loadings for this factor. These considerations make the interpretation of Factor C somewhat difficult, especially at the lower levels where the factor seems equally defined by all three tests. In the higher grades the definition of Factor C becomes easier; and we may tentatively regard it as Numerical Ability.

In the case of the boys and girls in grade 12 and the boys in grade 11 a fourth rotated factor was computed. In grade 12 it is labelled Factor D, and may perhaps be identified with Abstract Reasoning. Here it seems to be defined by the Abstract Reasoning test; but with the girls it also has high loadings in the Verbal Reasoning and Numerical Ability tests. Both the Verbal and the Abstract Reasoning tests call for formal rational solutions to abstract problems, though they do not require either syllogistic or spatial reasoning.

Factor E (grade 11) accounts for 12·8 per cent of the variance. It presents several features not shown by Factor D or by any of the other factors so far described. Its highest saturations (0·30 to 0·37) are found, not only in the tests for Abstract Reasoning and Numerical Ability, but also in the test for Clerical

Speed and Accuracy. On the whole therefore it would seem best to regard it as a factor for Perceptual Speed and Accuracy.

IV. DISCUSSION

In each group the principal axes solution obtained for the various intercorrelation matrices yields a first factor which has a higher saturation than any other factor in every test except the Clerical test. Since this is the only speed test in the battery, the factor may be identified with 'general ability', and apparently represents a general capacity for Reasoning analogous to that underlying the unrotated first factor observed by Doppelt (1950) and many earlier writers. Reference to the first columns of Tables I and II indicates that the pattern for this factor is essentially the same in every grade and for boys and girls. The 'coefficients of similarity' set out in Table V point to the same conclusion.

TABLE V. COEFFICIENTS OF SIMILARITY FOR THE FIRST PRINCIPAL FACTOR

			Boys					Girls		
Grade	8	9	10	11	12	8	9	10	11	12
Boys										
8		·996	·993	·992	·989	·994	·930	·937	·936	·933
9			·996	·993	·991	·995	·993	·999	·998	·995
10				·992	·992	·988	·994	·997	·996	·987
11					·999	·992	·997	·990	·997	·992
12						·991	·997	·988	·997	·993
Girls										
8							·994	·990	·994	·997
9								·990	·996	·990
10									·997	·989
11										·986
12										

Our main problem is to decide whether or not the influence of this general factor shows a consistent diminution with increasing age and grade. As will be seen from the figures at the foot of Tables I and II the amount of variance which it contributes to the total common-factor variance tends on the whole to decline. With boys indeed there is at first a slight but definite increase between grades 8 and 10; but after this there is a steady decrease (Table I). With the girls the figures show a consistent decrease throughout the grades (Table II). It is true that from one grade to the next the amount of decrease is not in every case statistically significant. Hence the evidence of this research, taken by itself, is not entirely conclusive. Nevertheless, so far as they go, the results would certainly seem to support the hypothesis that with increasing age and educational experience there is a gradual differentiation of ability. This is in keeping with results already reported by several earlier investigators (see Burt 1921 and 1954; Wright 1939; Garrett 1946; Anastasi 1948).

The amount of similarity between the factorial structure of two or more factor matrices after rotation has been assessed informally without using any very rigorous statistical test. The first two factors—those identified with general reasoning capacity and verbal ability—are defined by the same test variables in the higher grades as in the lowest grade of all—grade 8. This indicates that the pattern shown by these factors is much the same in all the groups irrespective of sex and school experience.

In the case of the number factor a similar conclusion does not hold good. The pattern changes from one grade to another, particularly with the girls. The factor composition of the eight tests changes still further when a new factor emerges at grades 11 or 12. However, the perceptual factor apparently shown by the boys in grade 11 does not reappear in grade 12. Instead a new factor emerges which seems to be associated with abstract reasoning ability. The emergence of yet another specialized factor at the higher grades seems analogous to the results obtained by Jones (1949) for children of 13: at this age a larger number of common factors was needed to account for the total variance of the Stanford-Binet test items. And a similar increase in factors was noted by Burt in analysing the earlier version of the Binet tests (1921, p. 266). In the present research, although the factor loadings for most of the tests fluctuate from one grade to another, they do not display any clear trend. Nevertheless, there is no indication whatever that the factor pattern tends to become simpler, as Chen' and Chow (1948) maintained.

We thus find that the number as well as the nature of the more specialized factors underlying all these tests for differential aptitudes are not precisely the same throughout the period studied, though there is considerable overlap at the lower levels. If we compare the factors found in the lower grades with those found in the higher grades, a marked difference is discernible. This is in accordance with the hypothesis that the differentiation of abilities is greater with older pupils than it is with the younger (Burt 1921, 1954; Garrett *et al.* 1935). The analysis of ‘ test specificity ’, however, fails to confirm the assumption of increasing specificity with increase in educational maturity. In Tables I and II the figures given in the column headed ‘ TS ’ vary from one grade to another in so haphazard a fashion that no generalization is here possible. Probably the initial data are insufficient to permit any decisive answer one way or another.

The analysis of the factorial structure of the various tests suggests that the similarities between the two sexes are on the whole more striking than the differences. The first two rotated factors agree fairly closely in regard to the tests which obtain the largest loadings for both sexes. Moreover, there is a close correspondence between the unrotated factor matrix for the boys and girls in different grades. Among the rotated factors the first (general reasoning) shows the least difference between the two sexes. The test for Mechanical Reasoning, however, shows increasing saturation with the general reasoning factor (Factor A) as the grade of the boys increases: with the girls, no such trend seems discernible. With the verbal factor, the largest difference between the two sexes is shown by the test for Verbal Reasoning; and in Table III the loadings for the Mechanical Reasoning decreases as the grade increases. This again is shown only by the boys. The number factor reveals a still wider difference between boys and girls in similar grades. This is especially marked at grades 8, 10, and 11. With the emergence of the additional factor in grade 11 (boys) the sex differences become far more pronounced.

The emergence of a new factor in the higher grades suggests that there is a greater differentiation of mental abilities for the older teenagers than for the young adolescents. This would seem to contradict the observation made by Doppelt that " the differentiation of abilities which is fundamentally a progressive reduction of the general factor in favour of specialization is as marked at the beginning of school years as it is at the end of those years " (Doppelt 1950, p. 73). In the present study Doppelt's conclusion holds true only so far as the boys and girls in grades 8 to 10 are concerned. During this period the number of factors found remains the same, and the factorial pattern does not greatly alter. Thus, although our results suggest much the same degree of trait differentiation irrespective of sex in every grade below 11, in the two highest grades there is some evidence for a decreasing integration of mental abilities. Further research therefore should seek to determine whether or not this tendency towards increasing specialization still continues during the following years, e.g. among college freshmen and sophomores, when adolescence is nearly over, and, if not, at what age or stage the tendency reaches its highest peak.

But the problem is decidedly complex; and there are many other problems still awaiting fuller examination. In the British investigation, cited above, two distinct questions were raised and two different methods employed. In the earlier inquiries the interest centred largely on scholastic abilities—notably verbal, numerical, and manual capacities—and turned largely on the cumulative influence of education, and particularly of specialized education at the later or ' secondary ' stage. The method adopted was to take cross-sections of the school population at successive ages. In other inquiries the tests were non-scholastic, and the interest centred rather on the apparent maturation of innate aptitudes as a spontaneous effect of mental growth. The method was that of the longitudinal study. The same individuals were tested and re-tested at intervals of two or four years. And what were compared were not school classes or grades, but complete age-groups. The inquiry reported in this paper was essentially a cross-sectional study: pupils in different grades were tested and compared. Further studies of the longitudinal type are urgently needed. The various abilities should be identified by marker tests; and tests of various abilities—observation, memory, imagination, and the like—should be applied as well as tests of educational abilities as such. A series of such inquiries should quickly lead to more decisive conclusions, and would be of the greatest practical value for both educational and vocational guidance.

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TABLE I. FACTOR LOADINGS FOR PRINCIPAL FACTORS : BOYS

Test	Grade 8								Grade 9								Grade 10							
	I	II	III	IV	h^2_o	h_i^2	SMC	TS	I	II	III	IV	h^2_o	h_i^2	SMC	TS	I	II	III	IV	h^2_o	h_i^2	SMC	TS
Verbal Reasoning	·740	·112	−·218	·090	·608	·583	·510	·27	·769	·074	−·084	−·077	·604	·600	·495	·28	·892	·078	−·090	·118	·811	·802	·719	·09
Numerical Reasoning	·709	·062	·171	−·005	·536	·520	·458	·31	·643	·038	·228	−·112	·466	·453	·370	·41	·662	−·118	−·225	−·076	·503	·474	·441	·39
Abstract Reasoning	·685	−·241	·162	·094	·553	·532	·464	·35	·706	−·286	·134	·023	·599	·570	·455	·26	·194	−·171	·061	−·101	·664	·651	·594	·24
Space Relation	·630	−·517	·079	−·034	·670	·617	·518	·26	·533	−·355	·066	·081	·414	·404	·307	·51	·769	−·282	·050	−·060	·673	·661	·579	·27
Mechanical Reasoning	·599	−·342	−·194	−·073	·513	·480	·414	·30	·553	−·372	−·261	·013	·513	·478	·338	·34	·682	−·342	·167	·109	·611	·598	·518	·25
Clerical	·120	·099	·365	−·032	·157	·124	·078	·61	·126	·126	·150	·131	·054	·051	·038	·78	·147	−·169	−·205	·059	·093	·061	·06	· 4
Language (Spelling)	·647	·515	·037	−·016	·685	·641	·549	·23	·567	·459	−·067	·051	·537	·495	·401	·38	·604	·515	−·038	·011	·632	·616	·504	·29
Language (Sentences)	·774	·288	−·094	−·062	·691	·667	·589	·17	·666	·358	−·073	·029	·577	·542	·450	·28	·729	·422	·112	−·026	·722	·693	·593	·16
Factor Variance (per cent)	79·4	19·6	6·10	0·7					80·0	19·7	5·1	1·3					84·3	15·8	3·3	1·1				

Test	Grade 11								Grade 12							
Verbal Reasoning	·833	·018	−·178	−·132	·743	·708	·641	·18	·864	·096	·090	·038	·765	·748	·678	·17
Numerical Reasoning	·775	·114	·219	−·105	·671	·637	·568	·27	·769	−·008	−·220	−·129	·657	·594	·554	·27
Abstract Reasoning	·690	−·161	·211	−·078	·553	·523	·469	·35	·689	−·234	−·147	·208	·595	·562	·489	·37
Space Relation	·720	−·321	−·037	·120	·638	·605	·537	·32	·689	−·369	·120	−·101	·635	·604	·522	·32
Mechanical Reasoning	·683	−·414	−·142	·024	·659	·629	·525	·19	·662	−·377	·141	−·081	·607	·583	·499	·25
Clerical	·270	·051	·182	·164	·135	·079	·111	·75	·314	·061	−·293	−·034	·190	·115	·159	·67
Language (Speillng)	·532	·493	−·104	−·009	·537	·516	·407	·38	·497	·627	·048	−·121	·656	·623	·487	·28
Language (Sentences)	·742	·332	−·049	·139	·683	·659	·541	·24	·744	·348	·115	·157	·712	·688	·590	·21
Factor Variance (per cent)	84·1	−15·4	4·4	2·2					80·2	19·1	4·8	2·6				

TABLE II. FACTOR LOADINGS FOR PRINCIPAL FACTORS : GIRLS

Test	Grade 8								Grade 9								Grade 10							
	I	II	III	IV	h^2_o	h^2_i	SMC	TS	I	II	III	IV	h^2_o	h^2_i	SMC	TS	I	II	III	IV	h^2_o	h^2_i	SMC	TS
Verbal Reasoning	·797	−·009	−·170	−·084	·664	·638	·596	·21	·791	·121	−·086	·009	·648	·632	·559	·24	·866	·031	·113	−·089	·765	·746	·675	·14
Numerical Reasoning	·711	·089	·191	·061	·550	·516	·481	·31	·651	−·041	·204	·146	·468	·458	·394	·35	·694	·122	−·177	−·080	·527	·501	·461	·34
Abstract Reasoning	·740	−·282	·040	−·142	·629	·625	·520	·29	·760	−·256	·030	·011	·645	·638	·535	·23	·810	−·257	−·124	·054	·739	·702	·629	·17
Space Relation	·562	−·255	−·045	·103	·382	·375	·321	·48	·660	−·374	−·079	·011	·582	·551	·463	·32	·687	−·356	−·027	·106	·600	·582	·514	·29
Mechanical Reasoning	·726	−·329	−·150	·084	·635	·622	·513	·05	·656	−·238	−·101	−·114	·498	·485	·420	·23	·600	−·294	·189	−·066	·482	·459	·395	·22
Clerical	·279	·041	·233	−·074	·134	·096	·098	·74	·309	−·062	·204	−·135	·141	·128	·108	·70	·099	−·017	−·227	−·062	·062	·020	·032	·85
Language (Spelling)	·679	·365	·105	−·018	·605	·588	·502	·30	·521	·472	·033	−·148	·495	·498	·371	·42	·640	·434	·039	·107	·599	·596	·482	·32
Language (Sentences)	·820	·335	·015	·052	·786	·771	·636	·10	·706	·428	−·071	·108	·687	·676	·517	·15	·780	·310	·036	−·009	·706	·701	·588	·15
Factor Variance (per cent)	88·5	12·0	3·2	1·4					82·6	16·10	2·8	2·1					87·1	13·5	3·5	1·1				

Test	Grade 11								Grade 12							
Verbal Reasoning	·902	−·049	·043	−·111	·818	·805	·742	·11	·839	−·009	−·092	−·097	·722	·695	·621	·21
Numerical Reasoning	·709	−·140	−·217	−·087	·569	·547	·499	·31	·704	−·092	−·215	·050	·554	·515	·464	·33
Abstract Reasoning	·743	·247	−·166	·078	·640	·634	·549	·27	·624	·216	−·118	−·061	·452	·440	·376	·40
Space Relation	·675	·501	·096	·118	·716	·701	·548	·20	·489	·478	·206	·027	·518	·481	·351	·40
Mechanical Reasoning	·608	·310	·111	−·132	·478	·483	·419	·24	·639	·431	·038	·069	·600	·583	·446	·09
Clerical	·220	−·034	−·085	·100	·057	·063	·057	·80	·229	−·136	−·138	·149	·112	·080	·071	·79
Language (Spelling)	·584	−·462	·082	·070	·561	·541	·475	·36	·598	−·418	·196	·084	·578	·547	·458	·34
Language (Sentences)	·796	−·365	·096	·053	·775	·746	·670	·11	·758	−·384	·122	·077	·743	·712	·597	·24
Factor Variance (per cent)	82·2	17·2	2·7	1·7					79·7	20·0	4·6	1·47				

TABLE III. LOADINGS FOR ROTATED FACTORS : BOYS

Test	FACTOR A					FACTOR B					FACTOR C					FACTOR D	FACTOR E
	General Reasoning Ability					Verbal Ability					Numerical Ability					Abstract Reasoning	Perceptual Speed
Grade	8	9	10	11	12	8	9	10	11	12	8	9	10	11	12	Grade 12	Grade 11
Verbal Reasoning	·407	·518	·541	·615	·583	·660	·559	·639	·553	·570	—·077	·152	·332	·225	·267	·167	·092
Numerical Reasoning	·433	·450	·422	·390	·508	·515	·331	·352	·461	·351	·288	·393	·449	·414	·525	·014	·368
Abstract Reasoning	·642	·715	·677	·515	·565	·303	·189	·373	·207	·139	·223	·229	·257	·369	·374	·340	·329
Space Relation	·808	·633	·714	·723	·770	·104	·063	·273	·173	·121	·085	·099	·298	·084	·162	·029	·281
Mechanical Reasoning	·650	·660	·741	·787	·760	·258	·194	·162	·121	·105	—·154	—·201	·189	·090	·130	·043	·129
Clerical	·016	·008	·098	·101	·104	·066	·103	—·019	·124	·134	·391	·208	·287	·050	·400	·032	·327
Language (Spelling)	·052	·110	·128	·077	·005	·794	·688	·783	·708	·780	·227	·227	·041	·084	·204	·079	·148
Language (Sentences)	·303	·251	·335	·315	·334	·770	·683	·780	·681	·708	·079	·218	·020	·060	·195	·248	·342
Percentage of Common Variance	43·9	50·23	45·2	44·4	45·9	47·5	38·6	42·2	34·4	33·6	8·6	11·2	12·6	8·4	15·0	4·5	12·9

TABLE IV. LOADINGS FOR ROTATED FACTORS : GIRLS

Test	FACTOR A					FACTOR B					FACTOR C					FACTOR D
	General Reasoning Ability					Verbal Ability					Numerical Ability					Abstract Reasoning
Grade	8	9	10	11	12	8	9	10	11	12	8	9	10	11	12	Grade 12
Verbal Reasoning	·578	·519	·607	·560	·496	·557	·580	·626	·629	·446	·142	·204	·072	·328	·319	·395
Numerical Reasoning	·387	·407	·365	·301	·336	·442	·331	·545	·489	·380	·454	·439	·312	·490	·456	·297
Abstract Reasoning	·697	·679	·718	·613	·525	·258	·249	·356	·255	·180	·278	·349	·312	·446	·239	·295
Space Relations	·571	·720	·719	·820	·709	·193	·109	·210	·114	·086	·137	·228	·198	·173	·033	·007
Mechanical Reasoning	·730	·644	·659	·645	·737	·235	·218	·216	·210	·126	·216	·187	·036	·137	·173	·106
Clerical	·120	·178	·035	·096	·026	·121	·109	·033	·139	·159	·324	·312	·243	·168	·292	·028
Language (Spelling)	·220	·073	·157	·071	·112	·717	·681	·755	·725	·721	·206	·164	·069	·173	·205	·054
Language (Sentences)	·317	·269	·339	·281	·223	·743	·771	·761	·798	·760	·366	·145	·109	·245	·306	·146
Percentage of Common Factor Variance	45·3	46·6	47·0	42·6	40·8	39·6	39·3	45·9	41·9	36·1	15·1	14·1	7·1	16·4	14·5	8·6

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AN ALTERNATIVE TABLE FOR DETERMINING MEANS OF SECTIONS FROM A NORMAL DISTRIBUTION

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The following note gives a table for determining means of vertical sections of the normal curve when the data given consist of the proportions of the total population falling above and below the limits of the sections.

In a previous paper (Sandon 1961) a table was given for estimating the means of sections from a normal distribution in cases where the data furnished consist of (i) the lower boundary of the section and (ii) the width of the section. It was then assumed that both items were expressed in terms of the standard deviation. In his supplementary note, however, Professor Burt pointed out that in most investigations of this nature the data usually consist of percentages—most frequently perhaps the proportions of the population or sample falling on either side of the section. Accordingly, a further table has now been prepared for use in such cases and is set out below (Table I, p. 72).[1]

To illustrate its use let us first of all apply it to the same problem as that considered in the previous paper, namely, Burt's data for occupational classes (Burt 1961). His table gave the number of persons per mille falling into each social class (col. 1 of Table II below). From this it is easy to determine by progressive addition the cumulative percentages, i.e. the proportions falling above or below the limits of each class or ' section ' (cols. 2 and 3). On referring to Table I and interpolating where necessary we can then readily ascertain the corresponding mean for each section in terms of the standard deviation (col. 4), and from these we can calculate the theoretical mean in terms of the I.Q. (col. 5). For comparison, the empirical values, given by Burt and calculated directly from

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[1] The present table is similar to that drawn up by Professor Burt for teaching purposes at University College and referred to in the previous issue of this *Journal*, (*loc. cit.*, p. 121). In Table I it will be noted that, if the figures for the means are read in oblique lines running from a particular percentage in the left hand margin to the same percentage in the top margin, they are symmetrical except in sign: e.g., −19·5, ·000, +·195; −·350, −·163, ·000, + ·163, + ·350; etc. In Professor Burt's table the means were arranged in a pyramidal array to bring out this symmetry. For the elementary student such an arrangement is doubtless more illuminating as it implicitly indicates the theoretical basis and emphasizes the way in which, with equal percentage differences, the means become more and more spread towards the extremes. However, for purposes of reference and research the arrangement adopted in Table I would seem to be more convenient. Here the figures are arranged instead as a right-angled triangle, truncated on the right; in view of the limitations of space and the duplication of the figures it appeared unnecessary to print the diminishing columns for ' the percentages below the section ' when they rise above 50: these, when needed, the reader can readily furnish for himself by applying the principle of symmetry. I am much indebted to Professor Burt for these and other comments and suggestions.

TABLE I. MEANS OF SECTIONS OF NORMAL CURVE

Percentage above Section	Percentage below Section										
	0	5	10	15	20	25	30	35	40	45	50
0	0·000	+0·109	+0·195	+0·274	+0·350	+0·424	+0·497	+0·570	+0·644	+0·720	+0·798
5	−0·109	0·000	+0·085	+0·163	+0·236	+0·307	+0·376	+0·445	+0·515	+0·585	+0·657
10	−0·195	−0·085	0·000	+0·058	+0·149	+0·219	+0·287	+0·354	+0·422	+0·490	+0·559
15	−0·274	−0·163	−0·058	0·000	+0·072	+0·141	+0·208	+0·274	+0·340	+0·407	+0·474
20	−0·350	−0·236	−0·149	−0·072	0·000	+0·069	+0·135	+0·201	+0·266	+0·331	+0·397
25	−0·424	−0·307	−0·219	−0·141	−0·069	0·000	+0·066	+0·132	+0·196	+0·260	+0·325
30	−0·497	−0·376	−0·287	−0·208	−0·135	−0·066	0·000	+0·065	+0·129	+0·192	+0·256
35	−0·570	−0·445	−0·354	−0·274	−0·201	−0·132	−0·065	0·000	+0·064	+0·127	+0·190
40	−0·644	−0·515	−0·422	−0·340	−0·266	−0·196	−0·129	−0·064	0·000	+0·063	+0·126
45	−0·720	−0·585	−0·490	−0·407	−0·331	−0·260	−0·192	−0·127	−0·063	0·000	+0·067
50	−0·798	−0·657	−0·559	−0·474	−0·397	−0·325	−0·256	−0·190	−0·126	−0·067	0·000
55	−0·880	−0·732	−0·629	−0·542	−0·463	−0·390	−0·321	−0·254	−0·189	−0·126	
60	−0·966	−0·809	−0·703	−0·613	−0·532	−0·457	−0·386	−0·319	−0·253		
65	−1·058	−0·891	−0·780	−0·686	−0·603	−0·526	−0·454	−0·385			
70	−1·159	−0·978	−0·861	−0·764	−0·677	−0·598	−0·524				
75	−1·271	−1·073	−0·949	−0·846	−0·756	−0·674					
80	−1·400	−1·179	−1·045	−0·936	−0·842						
85	−1·554	−1·300	−1·141	−1·036							
90	−1·755	−1·447	−1·282								
95	−2·063	−1·645									

Percentage below Section	Percentage above Section (to complete col. 1)				
	95	98	99	99·5	99·9
0	−2·063	−2·421	−2.665	−2·892	−3·367

the test-measurements, are shown in the last column (col. 6). It will be noted that there are appreciable discrepancies in several of the higher and lower classes. In a personal communication Professor Burt explains that the distributions *within* the higher and lower classes are in fact appreciably skewed, i.e. there is a longer tail stretching upwards or downwards towards the mean of the whole population (100 I.Q.), and he states that the theoretical ' means ' as here calculated would give a much closer approximation to the modal or median mark of each occupational class.

TABLE II. DISTRIBUTION OF INTELLIGENCE ACCORDING TO OCCUPATIONAL CLASS

Mean I.Q's calculated (i) from Percentages, (ii) from Individual Measurements

Class	Percentage (Burt) (1)	Cumulative Percentages (a) Above Section (2)	(b) Below Section (3)	Mean in S.D. (4)	Mean in I.Q. (Calculated) (5)	Mean in I.Q. (Burt) (6)
I	0·3	0·0	99·7	+3·05	145·7	139·7
II	3·1	0·3	96·6	+2·14	132·1	130·6
III	12·2	3·4	84·4	+1·34	120·1	115·9
IV	25·8	15·6	58·6	+0·58	108·7	108·2
V	32·5	41·4	26·1	−0·20	97·0	97·8
VI	26·1	73·9	0·0	−1·24	81·3	84·9

This method is also appropriate in those cases in which the classification is based, not on numerical marks, but on a qualitative distinction, expressed, it may be, in terms of literal marks (alpha, beta, gamma, etc.) or in terms of ordinal classes. To illustrate such a case I shall take the frequencies given for the classes allotted by five examiners who marked the same set of English essays submitted by 50 candidates at an " Entrance Scholarship examination for a group of colleges in a University " (Hartog, *et al.*, 1936, p. 144, Table 97). The numbers allocated by examiner A to each of the four ' classes ' and the working for his results are set out in Table III.

The examiners were also asked to assign a numerical mark (on a scale with a maximum of 100) to each of the candidates. Here therefore we have a basis of comparison and an instructive check. Dr. Rhodes (*op. cit.*, p. 228) tells us that the average for the ' ideal marks ' for the essays would be approximately 52 and the standard deviation approximately 10 (52·5 and 9·8 to be precise). I have therefore multiplied the marks in column 4 by 10 and added 52. The values thus obtained may then be compared with the mean of the marks for each class as awarded by the original examiners. The examiners' means can readily be calculated from the complete class list printed in Table 96 (*loc. cit.*, p. 143): they are given in column 6 in Table III.

Table IV shows the mean marks for all examiners computed in the manner described from the values obtained with the aid of Table I. It will be seen that the means for the several classes, as implicitly assumed by the different examiners, differ appreciably, particularly in the case of classes I and IV. For example, a borderline candidate (like number 8 in Table 96 who usually gets 68 to 70, and obtains a first class with examiners B, C, and E, but only a second class with examiner D) virtually loses 14 marks with examiner D's classification. This illustrates a conclusion which I have already stressed in previous papers—namely, that where candidates are placed in classes it is desirable to use as many classes or categories as possible (e.g. by appending a plus or a minus) especially when the marks for one subject are to be pooled or averaged with those for others (cf. Sandon 1959).

TABLE III. COLLEGE ENTRANCE EXAMINATION : ENGLISH ESSAY

Classes and Marks awarded by Examiner A

Class	Percentage (1)	Cumulative Percentages (*a*) Above Section (2)	Cumulative Percentages (*b*) Below Section (3)	Mean in S.D. (4)	Mean in Marks (Calculated) (5)	Mean in Marks (Actual) (6)
I	14·0	0·0	86·0	+1·60	68·0	69·3
II	48·0	14·0	38·0	+0·33	55·0	56·0
III	30·0	62·0	8·0	−0·79	44·1	43·4
IV	8·0	92·0	0·0	−1·80	34·0	28·5

TABLE IV. COLLEGE ENTRANCE EXAMINATION : ENGLISH ESSAY

Theoretical Mean Marks for Various Classes Calculated from Frequencies

Class	Examiner A	B	C	D	E
I	68	67	70	74	69
II	55	55	54	57	56
III	44	45	42	41	45
IV	34	35	28	31	34

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Vol. XV Part 1 — The British Journal of Statistical Psychology — May 1962

NOTES AND CORRESPONDENCE

ROTATION OF AXES AND PSYCHOLOGICAL SPACE

To the Editor, British Journal of Statistical Psychology

SIR,—In his note on the above subject (this *Journal*, XIV, 1961, p. 109) Dr. Jarrett does well to bring us sternly face to face with those fundamentals which factor analysts are prone to take for granted. I do not think, however, that the situation is as bad as he suggests. In the first place, although he speaks of the glib use of standard scores (implicit in all correlation analysis), his argument and in particular his examples proceed as if factor analysts did not in fact use them. His first example of the height-weight bivariate scattergram and the meaning of any measurements along its major axis, and his later ' hypothetical example ' of the three-dimensional scheme of religious devoutness, difference limen, and oral fixation, produce the absurdities he speaks of only if each variable is plotted in terms of the units in which each was originally measured. As Dr. Jarrett both knows and states, factor analysts do not work in this way, except in certain special cases where there is no trouble over metric, and they can therefore work with co-deviance or co-variance rather than correlation matrices; and it is precisely because they realize the difficulties arising from differences of metric that they choose to work in standard scores and correlation matrices.

Dr. Jarrett's argument apparently is that this only clouds the issue since standard scoring changes the scale without materially changing the unit. That, I submit, is not true. A standard score being a ratio is simply a number and has no dimensions, as a glance at its formula will show. A correlation being the mean product of such dimensionless numbers must similarly be dimensionless. The difficulty of which Dr. Jarrett speaks arises, I think, from the practice of representing a dimensionless standard score by a length. This is a common practice even at the most elementary levels of arithmetic, and there too leads to confusion. Thus the simple demonstration $2 \times 3 = 6$ (all numbers dimensionless) is often made at junior school level by drawing at right angles a couple of lines, one of two units and the other of three, and then showing that the rectangle included contains six squares. One can easily imagine that somebody concerned with fundamentals might enquire into the nature of the units appropriate to these six squares. The conflict of units between the six on the one hand and the two and the three on the other arises only from the practice of using, for the purposes of convenience or demonstration, a length to signify a number. The trouble with which Dr. Jarrett deals is, I suggest, essentially of the same nature.

In itself factor analysis is not concerned with any kind of space. The space in terms of which factor analysts for purposes of convenience are accustomed to think is merely a fiction. It is a useful fiction, provided that one takes care not to press the analogy with ordinary three-dimensional space, or with the mathematicians' higher dimensional space, too far. If, however, one remembers that the various lines merely represent dimensionless numbers, then the problems which Dr. Jarrett speaks of do not arise. As a representation of the relations between dimensionless numbers the factor analytic space is isotropic; and the first of the two alternatives suggested in the second paragraph on page 113 (viz., that operationally different dimensions such as religious devoutness, visual difference limen, and oral fixation can in principle be measured by the same operation) is a perfectly reasonable one.

G. E. R. BURROUGHS

(Dr. Jarrett's reply will appear in the next issue. *Editor*.)

FREQUENCY CURVES AND THE ABILITY OF NATIONS. I

Sir,—There would seem to be some inconsistency in the assumptions adopted by Professor Burt, Miss Howard, and others in their recent discussions of the frequency-distributions obtained with tests and other assessment for general intelligence. In their earlier account of multifactorial inheritance both Miss Howard and Professor Burt assumed that the result of this mode of transmission will necessarily be a normal distribution of individual differences, since the normal curve represents the limit of the symmetrical binomial (this *Journal*, IX, 1956, pp. 95f.). In his report on *The Distribution of Educational Abilities* (p. 23), Burt even declares that, owing to the normality of the distribution, it is possible, " when dealing with large numbers, to predict the probable number of children falling within or beyond certain specific limits of ability with the same accuracy as we can foretell the proportion of heads in tossing coins or the scattering of shots fired from a gun ". Educational administrators can therefore use the ' curve or errors ' as a means of computing the accommodation needed for pupils of different types—special schools for the defective, central schools for the bright, classes for the educationally subnormal, and scholarships for the supernormal. Later (p. 47) we are told that the curve also supplies the practical teacher with a convenient means of standard scales of marks. Galton and Pearson, as has so often been remarked, were blind devotees of the normal curve; but this goes further still.

In her paper on the ' Conversion of Scores to a Uniform Scale ' (this *Journal*, XI, 1958, pp. 199f.) Miss Howard also adopts Galton's assumptions, and takes it for granted that the ideal or theoretical distribution for complete age-groups will be strictly normal. It follows that, if the assessment or scores furnished by the teacher are skewed or otherwise non-normal, they ought first to be re-scaled so as to conform with the requirements of normality.

So far therefore Mr. Pidgeon and other critics would appear to be perfectly justified when they contend that the normal distribution exhibited by the psychologist's measurements of children's intelligence is artificially imposed. However, in his article on ' The Distribution of Intelligence ' (*Brit. J. Psychol.*, XLVIII, pp. 161f.) Professor Burt takes a very different line. He repeats the definition of intelligence given in his Galton lecture; but he now rejects Galton's assumption that intelligence is distributed normally: instead he maintains that the distribution conforms with something that he calls ' Type IV '. What precisely ' Type IV ' denotes, or why intelligence should not be distributed in conformity with Types I, II, or III (whatever they may be) is never explained. He does indeed print a diagram showing the actual distribution of measurements for intelligence obtained from two thousand children; but I confess that most of us, judging solely by inspection, would be quite prepared to accept it as a good approximation to the normal curve—certainly as good as we have any right to expect from mental measurement, which themselves are necessarily approximate. Burt gives values for three coefficients—two ' beta ' coefficients and one designated ' kappa ' (which again are not defined), and tells us that these show that the curve is in fact skewed and ' leptokurtic '. The last statement is doubly puzzling. In Aitken's *Statistical Mathematics* (p. 38) it is explained that " curves for which $\beta_2 < 3$ are leptokurtic and those for which $\beta_2 > 3$ are platykurtic ". But the value Burt gives for β_2 is 3·36. Surely therefore the curve should be described as ' platykurtic ' not ' leptokurtic '. In any case the disparities seem almost negligible. Why should so much stress be laid upon them?

Both the terminology and the discussion are too technical for a ' general journal ' of psychology; and even post-graduate research students can hardly be expected to familiarize themselves with all the recondite mathematical details of Pearson's twelve-fold classification of curves. The statistical journal, however, claims to publish " elementary papers of an expository nature ". Cannot Miss Howard or one of her collaborators therefore give us a brief summary of what is necessary, and adapt her explanation to the level of those who never got beyond the early stages of schoolboy algebra and calculus, and whose memory even of those has become decidedly rusty?

In his recent Conference paper on ' Galton's Contributions to Psychology ' (*Bulletin*, XLV, pp. 10–22) Professor Burt seems to revert to the theory that the principles of ' multifactorial inheritance ' leads to a purely ' normal frequency distribution ' for graded characteristics, such as stature and intelligence. However, he starts by announcing that his object is to attempt an appraisal of Galton's views in the light of present-day psychology, now that half a century has elapsed since his death. If, in the view of Burt or others, the normal curve no longer holds good, then we should have expected to hear something about the recent change in theory.

Galton himself seems to have felt so sure of the normality of the distribution that he used it as a basis for comparing the ability of nations. Periclean Athens, he tells us, produced men whose ability would rank in this country as class H (1 in a million) and produced at a rate with which we produce men of class F (1 in four or five thousand): H is two grades (i.e., roughly two ' quartiles ' or ' probable errors ') above F; hence, he concludes, the average level of Periclean Athens must have been two grades above the average level of Victorian England.

This brings me to a further point of criticism. The most glaring omission from Burt's conference paper is the absence of any attempt to evaluate the particular doctrine that Galton himself so strongly upheld—the doctrine that it is the innate ability of each nation that is chiefly responsible for its success or failure in the struggle for survival. At the outset of his remarks, it is true, Burt mentions the fact that Galton was popularly regarded as ' the apostle of eugenics '. But it seems regrettable that more space was not devoted to this subject. Eugenics are dismissed almost at once with a jocular reference to Tenniel's cartoon; and throughout the remainder of the discussion no attempt whatever is made to state, defend, or criticize Galton's own arguments. Yet this is the most controversial of all Galton's contributions to psychology, and the one that most loudly calls for reappraisal.

My final question therefore on this aspect of the matter is simply this: what precisely *is* the present-day attitude of psychologists, historians, and biologists to this genetical interpretation of ' the rise and fall of nations '? If we accept the views of Burt and Miss Howard, Galton's notions of mental inheritance must now be out of date. Galton knew nothing of the Mendelian theory, and still less of the theory of ' multifactorial transmission'. It is therefore no longer possible to accept, as contemporary writers in the *Eugenics Review* so often seem to do, Galton's facts and explanations as furnishing decisive evidence for demonstrable conclusions or to endorse his eugenical blue-prints for a future Utopia. McDougall admits that there was " a gap in the eugenist argument that could only be filled by the method of experimental psychology "; but he claims that " the gap has been bridged by the investigations of Burt, Miss Arlitt, and H. B. English ".[1] But McDougall's mode of reasoning seems even more fallacious; he assumes throughout that there is a sharp and readily recognizable distinction between the effects if heredity and the effects of culture and environment.

Burt attempts to give McDougall's argument a Mendelian twist. The Greeks, he supposes, were a very mixed stock; and mixed or mongrel races, he assumes, are superior to the thoroughbreds. But he offers no factual evidence in support of either assumption. If there are *actual facts* that bear on these two basic premises, may we ask him to enumerate them? As it is, I should have thought the origin of the ancient Greeks was lost in the mists of meaningless legend. When we consider the actual course of history—the rise and fall

[1] *National Welfare and Natural Decay*, pp. vii, 69f., 78f.; cf. also his *Group Mind*, pp. 247f. (on Greece) and 255f. (on Spain). I do not wish to deny the Mendelian hypothesis within the particular fields which Mendelian biologists have investigated. I am prepared to admit the Mendelian transmission of certain rare types of mental deficiency and of ' blood-types ' in normal human beings. But no one today seriously supposes that differences in ' blood ' have any real *psychological* effects. If Professor Burt has any evidence whatever for the alleged connection between ethnic differences in blood-groups, and ethnic differences in behaviour or any other ethnic implications, will he set forth his evidence in detail or at least cite the relevant references?

of Greece, of Rome, of Islam, and of the various European nations (including ourselves)—surely it becomes obvious that the necessary conditions for success or failure were in every case very much the same. The obvious and familiar reason for the ascendancy of the ancient Greeks was simply the *migration* of the Greek race into the Greek peninsular at the dawn of their history, and, as a consequence of the migration, *conquest*, followed in the natural course of things by the confident sense of peace and security that a victorious nation always enjoys and the opportunities for cultural development that such a peace always affords. In like manner the necessary causes of their subsequent decline and fall were simply the expansion and the conquests of another still more powerful nation. Nor can we infer from the mere fact of conquest or of successful migration into a new country or new class that the successful stock enjoyed some inherent superiority: they may simply have profited by some temporary superiority acquired from the stage of civilization they may happen to have attained. And in countries of mixed race, like Britain or the States, all the immigrants contribute something to the common culture: end it would be invidious to discriminate.

But it is hardly necessary to turn back to ancient history. In our own day we have seen much the same thing happen. We have watched Prussia, after conquering much of Europe and annexing provinces from defeated France, start colonizing overseas (Galton's Damaraland, for instance), and then seek *Lebensraum* in the East. We have witnessed a whole race persecuted, and many of the most highly cultivated men—non-Jews as well as Jews—forced to emigrate overseas, or actually deported to Russia or elsewhere. To this some have assigned (among other results) the marked deterioration in the quality of German post-war contributions to science and the improvement of Russian and American technology. But in all such changes, social, political, and other environmental factors, are inextricably involved; and it is quite impossible to discern in this direction any verification of Galton's theories, but rather the reverse. The transfer of knowledge or skill is not a transfer of biologically inheritable capacity.

The problem of individual *differences*, as psychologists prefer to describe it, is simply the obverse, of what sociologists and economists prefer to call the problem of *equality*; and from this alternative standpoint by far the best analysis (ignored by most psychologists) is that contained in Professor Tawney's well-known volume with that title. It is indeed difficult to avoid the conclusion that Galton's present-day followers are in effect seeking to revive what Tawney has stigmatized as 'the religion of inequality'. This does not mean (as Miss Howard apparently assumes) that those who favour an environmentalist interpretation want to deny outright the existence of *any* innate differences between individuals. Tawney indeed in his very first chapter cites "Dr. Burt's admirable studies of the distribution of abilities among school children" as showing that "quite apart from differences of environment and opportunity, individuals differ widely in their natural endowments; . . . although 80 per cent at the age of 10 fall within a range of three mental years, the most backward may have a mental age of 5, and the most gifted one of as much as 15." This is Tawney's own summary of the facts; and it is, he adds, "not open to question".[1] He even acknowledges that "the political and psychological theory of the age between 1750 and 1850" (i.e., before Darwin and Galton published their views) "greatly underestimated the significance of inherited qualities and greatly overestimated the plasticity of human nature".

Yet these concessions do not mean that we are ready to allow the hereditarian theorist to attribute practical all important differences between individuals, families, social classes, and nations, or between the same nation at different times solely or mainly to innate differences in ability, as Burt appears to do in his discussions of social mobility (this *Journal*, XIV, pp. 3f.). Dr. Maxwell wants to go further still: he even attributes the 'neurotic tendencies' exhibited by certain children to 'genetic factors' revealed by the WISC battery, which he supposes have been produced by 'pronounced assortative mating.'

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[1] R. H. Tawney, *Equality*, p. 36; cf. also p. 150.

The most casual observer cannot deny the existence of wide differences in economic background, educational opportunity, social status, and in the numerous privileges and advantages that spring from social status. Why then should our twentieth century Galtonians assume that these observable differences have a smaller influence than the hypothetical differences they ask to postulate in genetic constitution? As Tawney very justly observes, " the diversities between individuals are less important and fundamental than their common humanity"; and what he and others "deplore is not inequality of personal gifts, but inequality in social, educational, and economic environment". It is precisely this second type of inequality that Galtonians, it would seem, wish to justify and maintain. Free play for social mobility is no doubt desirable; but it is neither the only corrective nor the most essential. What is most urgently needed, as Tawney argues, are not merely opportunities for a few to ascend, but " a high level of *general* culture ".

As it is, the discrepancies between potential ability and actual reward are out of all proportion to the alleged differences. " Five and a half per cent of the population ", we are told, "takes forty four per cent of the national income " (nearly one half) "leaving only fifty six per cent of the income to ninety four and a half per cent of the population " (Tawney, *op. cit.* p. 68). Miss Howard assumes that ability is distributed according to a normal or bell-shaped curve; Professor Burt corrects this by saying that the distribution is of Type IV with a slightly longer tail extending downwards. But, if the figures just quoted are correct, the distribution of personal incomes has an enormously long and attenuated tail towards the *upper* end of the scale: indeed it would seem to be almost J-shaped. Surely it becomes quite impossible to account for this second man-made distribution by invoking the first or natural distribution, no matter what play we allow to ' social mobility '.

In his references to rise in social rank Galton carries his denial of environmental influences to an extreme degree. " Few who possess these very high abilities " (so he confidently asserts) " can fail to achieve eminence ". And in much the same spirit Miss Howard and Miss Conway (this *Journal*, XII, pp. 5–14) declare that " in this country there has *always* been a tendency for able youngsters to rise in social rank " (my italics), so that "all ranks of society, from the villein to the monarch, form a far more united community than the critics of class distinctions would have us believe ". If there is any evidence in British history for such an assumption, could we be given full details? I should have thought that (with rare exceptions) only during the present century—indeed only since the Education Act of 1944—have boys of humble origin been really free to climb the educational ladder, and so get to the top of the social tree, or at least as near it as their industry and their ambitions deserve.

All these questions seem to be closely connected and of considerable practical importance. And yet the few psychologists who have touched upon them seem to offer us no more than ill substantiated and inconsistent guesses.

J. F. Isaacs

FREQUENCY CURVES AND THE ABILITY OF NATIONS. II

Dr. Isaacs has raised a number of related yet very different problems, which are all of great practical interest, but difficult to answer satisfactorily in the space at my disposal. May I begin with his question about frequency curves?

The Observed Distributions. Here the points which Dr. Isaacs emphasizes certainly deserve far more attention than they have hitherto received; and a clearer and more comprehensive statement of the present position, in terms which the general student of psychology can understand, is much to be desired. In replying to the questions which he has addressed more particularly to me I shall therefore attempt a general outline of the subject as I see it, largely in the hope of provoking a more detailed discussion from the various specialists.

The problem that seems chiefly to puzzle Dr. Isaacs can readily be answered. So far as intelligence is concerned, the mental differences between individuals exhibit a frequency-distribution which is certainly not *exactly* normal, but nevertheless approximates to it sufficiently well for the normal curve to be used for a number of theoretical and practical purposes. The main reasons for using it, as will be seen in a moment, are first that it obeys an equation which is much simpler than any other frequency-curve, and secondly that both the ordinates and the areas corresponding to different values of the abscissa have been calculated and tabulated in great detail.

Consider first of all the simplest conceivable scheme of causes that might be advanced to account for the way individual deviations are distributed. Let us suppose that the causes which produce them are exceedingly numerous, but have each an exceedingly small effect, and that in the long run causes which tend to increase or to decrease a given deviation are equally probable; then from assumptions such as these (but rather more rigorously formulated) it is possible to show that the frequencies of the deviations would in fact obey what is commonly known as the 'normal law of error'. However, as experience has demonstrated, this extremely simple scheme is not likely to be exactly satisfied by the conditions obtaining in any observable field of study. In statistical theory at the present day, therefore, the chief interest of normal distribution lies, not so much in its applicability to distributions actually observed, but rather in its use in the theory of sampling.

Whether or not it is reasonably safe, in any particular discussion, to adopt for working purposes the simple but convenient model from which the normal distribution is derived must depend on the nature of the investigation. In the paper of mine which Dr. Isaacs quotes, I was thinking of fairly limited samples of children, attending ordinary elementary schools, on whom a teacher might be reporting, or whose performance the teacher might be attempting to assess. In such cases the normal distribution certainly appears to provide a satisfactory basic for scaling. It is chiefly when we are concerned with exceptional types of pupil—those who are markedly subnormal or supernormal—that a more precise and elaborate model becomes essential.

Dr. Isaacs goes on to quote from the L.C.C. report on the *Distribution and Relations of Educational Abilities*. This however, was published in 1917, and was therefore one of the earliest studies of the problem. Dr. Isaacs omits to point out that the advantages of assuming a normal distribution are there stated very hypothetically. The passage in question begins by emphasizing that the distributions actually observed conform only very *approximately* to the normal type, and that the tests for 'goodness of fit' showed that the departures, though comparatively slight, were nevertheless fully significant (*loc. cit.*, p. 34). Hence, as was repeatedly stated, predictions and calculations made on such a basis could only be regarded as rough and provisional.

The Genetic Hypothesis. The reasons why the observed distributions tend towards normality, and yet are never perfectly normal, have since become much clearer. First of all, in accordance with Mendelian principles, it is generally agreed that characteristics which exhibit a graded distribution must depend on the combined effects of a large number of genes whose separate influence is small but cumulative. This by itself would produce a frequency distribution indistinguishable from the normal. To explain the enlarged lower tail of subnormal or mentally deficient individuals it is frequently suggested that various pathological factors may be operative—either before, during, or just after birth. That no doubt is part of the explanation; but it is now quite evident that it is by no means the whole explanation. There are numerous special types of mental deficiency which, as the pedigrees indicate, are due to single genes. Thus in a report of the Medical Reseach Council it is estimated that out of all surviving cases of low grade mental defects at least 20 per cent—and possibly twice that number—are due to ' rare recessive genes ', and another 4 per cent to ' rare dominant genes '.[1] It is unlikely that these are the only types of marked deviation produced by single genes. Owing to the concomitant physical abnormalities which are also caused by the genes such cases are naturally the easiest to discover. Moreover, if certain mutations can produce unfavourable deviations, it is probable that others may occur which would produce favourable deviations. And why should we assume that there is a sharp distinction between the polygenes of small effect and the major genes of large effect? It is far more likely the observable effects of different genes or of different gene-mutations vary from small to great. Nevertheless, in the *majority* of cases the effects of a single gene are undoubtedly small; and whether or not the effect of some particular gene-mutation will be favourable or not will be mainly a matter of chance. If stability has already been achieved, or if the conditions for successful adaptation are at all complex, then the effect of any new mutation is more likely to be indifferent or harmful than beneficial. But if its effect is *very* small, it can hardly be *very* harmful; and it is by the progressive selection or elimination of quite small differences that evolution has proceeded. The effects of some genes appear to be simply additive. But often some may be dominant, others recessive; and in many cases the ultimate effects may depend largely on interaction or correlation between the genes—one gene may be necessary for the manifestation of another, or it may inhibit its manifestation, or their occurrence may be linked, and so on. Nevertheless in spite of these detailed complications, the final outcome may be expressed to a close approximation as the additive result of major and minor causative processes of varying intensity, as Fisher has shown.[2]

On the basis of these various considerations we may now construct a plausible theoretical model to explain the frequency-distributions actually encountered. As will be seen, some of the assumptions have a sound basis in current genetical observations or theories; others, though not so well established, appear highly plausible. So far then as individual differences in intelligence are attributable to genetic influences the essential assumptions may be summed up as follows.

(i) The differences between individuals are due to the combined effect of a large number of genes of varying effect.

(ii) Polygenes producing small deviations are far more numerous than major genes producing large deviations.

(iii) Polygenes and major genes may produce either favourable or unfavourable deviations.

(iv) Major genes producing unfavourable deviations are more numerous than those producing favourable deviations.

(v) For purposes of computation the final effects of the genes and of the processes they set up may be treated as the additive result of a large and indefinite number of minor contributory processes obeying the usual laws of probability, but not entirely independent of one another.[1]

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[1] Medical Research Council, *The Hazards of Man of Nuclear and Allied Radiations* (1956). Most of the clinical types attributed to this type of causation are described in L. S. Penrose's *Biology of Mental Defect* (1949).

[2] For a further discussion of the mathematical formulation see this *Journal*, IX, pp. 120f.

The type of distribution that this model will produce is obvious. It will be bell-shaped, and more or less asymmetrical; both tails, but especially the lower tail, will be markedly prolonged; and since—no matter how large a given deviation may be in one direction—we can always imagine that one more gene or gene-mutation will produce a deviation larger still, it follows that the range will be unlimited in both directions.

Frequency Curves: Their Types and Formulae. The next problem therefore will be to consider how these specifications can be expressed by an algebraic formula, such as will enable us to study the properties of the curve and to calculate the theoretical frequencies. We have consequently to ask how such distribution curves are to be distinguished, classified, and described. For this purpose we shall have to glance briefly at the scheme proposed by Karl Pearson, so far as it is relevant to the work of the educationalist and psychologist; and this will incidentally allow us to answer Dr. Isaacs' inquiries as to what is meant by a Type IV curve, and when precisely it should be adopted in preference to the normal curve or some other type, in problems relating to the distribution of general intelligence.

Frequency-curves for measurements that involve no mixture of species or types are generally unimodal. The rate of change in the frequencies (dy/dx) increases and decreases sometimes directly as the measurements (x) increase or decrease, and sometimes inversely. It is zero both when the frequency (y) is zero and when it is a maximum; and the maximum is attained when the measurement is at or near the average, i.e. when $x=a$ (say). These various conditions will evidently be satisfied by an equation of the general form

$$\frac{dy}{dx}=\frac{(x-a)y}{f(x)}.$$

Let us expand the function $f(x)$ in a series of powers by Maclaurin's theorem, and discard terms involving x^3 or higher powers, since they are difficult to determine with accuracy and curves involving them would be of decidedly unusual shape. Our equation can then be rewritten (with a change of origin)

$$\frac{dy}{dx}=\frac{(x+a)y}{b_0+b_1x+b_2x^2}. \tag{1}$$

We may note three types of case straight away.

(1) If both b_1 and b_2 are zero, the curve obtained will give a reasonable fit for the symmetrical binomial where (as in tossing coins) the chances for and against one of the two possible results are equal. The curve itself, as we shall see in a moment, then becomes the so-called 'normal curve'.

(2) If only b_2 is zero, the curve obtained will fit the asymmetrical or 'point' binomial, where (as in throwing dice) the chances are unequal. The asymmetry may be so extreme that the curve is J-shaped.

(3) If none of the three constants are zero, the curve obtained will fit the so-called hypergeometrical series. In this the successive terms represent the chances of getting $n, n-1, \ldots, 0$ black balls from a bag containing pN black and qN white balls, when n balls are drawn one by one without being replaced. It in fact is the frequency-distribution which arises when we do *not* assume that the causes at work are independent of each other or of equal valency, or that the contributions in excess of defect are equally probable. The formula for the hypergeometric distribution is given, and discussed with detailed illustrations, in Appendix III of Burt's *Backward Child*, 1937, pp. 664f. The resulting curves are usually bell-shaped; but in certain cases they may be either J- or U-shaped. On considering the

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[1] This summary of the essential assumptions is due to Professor Burt.

second derivative, we find that the point or points of inflection are given by $x^2 = b_0/(b_2 - 1)$. There are thus at most only two such points; and if there are two, they will be equidistant from the mode.[1]

In order to plot the curve we must change eqn. (1) to an equation having the form $y = F(x)$, and to do this the expression on the right must be integrated. For example, consider the simplest conceivable case, namely, that in which $b_1 = b_2 = 0$. Measure x from the mode so that $a = 0$, and use a scale which makes $b_0 = -1$. Then

$$\frac{dy}{dx} = -xy \text{ or } -x = \frac{1}{y}\frac{dy}{dx}. \tag{2}$$

Now, as the most elementary schoolbook on the calculus will explain, this last expression $= d \log y/dx$; and on integrating we have

$$\log y = -\tfrac{1}{2}x^2,$$

and hence[2]

$$y = \exp\left(-\tfrac{1}{2}x^2\right). \tag{3}$$

This is the equation for the normal curve, as we shall see in a moment.

When the three coefficients in the denominator of the right hand expression in eqn. (1) above, namely, b_0, b_1, and b_2, *all* have non-zero values, the integration is more complicated and the particular form taken by the integral will depend on the nature of that expression. It will be seen that it can in fact be written as follows:

$$b_2\left[x - \frac{-b_1 + \sqrt{(b_1^2 - 4b_0b_2)}}{2b_2}\right]\left[x - \frac{-b_1 - \sqrt{(b_1^2 - 4b_0b_2)}}{2b_2}\right] \tag{4}$$

Evidently therefore the criterion for fixing the form in any specific case will be identical with the criterion[3] for the nature of the roots of the quadratic equation $b_0 + b_1x + b_2x^2 = 0$, namely,

$$k = b_1^2/(4b_0b_2). \tag{5}$$

The criterion subdivides the whole field into (*A*) three main types and (*B*) three limiting types according as k takes the one of the values 0, 1, or ∞, or lies somewhere in between. The classification which thus results is shown diagrammatically in Figure 1 (p. 86, below).

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[1] The formula for the hypergeometric series is

$$f = \frac{1}{N^{(n)}}\sum_{r=0}^{n}\left\{\binom{n}{r}(Np)^{(r)}(Nq)^{(n-r)}\right\},$$

where N denotes the number of individuals in the population, n the number in the sample drawn from it, p the chance of a 'success' and $q = (1-p)$ the chance of a 'failure'. The notation $N^{(n)}$ etc. denotes $N(N—1)\ (N—2)\ \ldots$ to n terms. The analogy with the formula for the binomial distribution is patent. For a fuller and more technical discussion of the distribution see Kendall's *Advanced Theory of Statistics*, 1943, I, pp.126f.

[2] In teaching these formulae to my younger pupils I ask them to substitute the value thus reached (3) in the initial formula (2). With a little help they then easily see that

$$\frac{dy}{dx} = \frac{d\left[\exp\left(-\frac{1}{2}x^2\right)\right]}{dx} = \frac{d\left(-\frac{1}{2}x^2\right)}{dx}\,.\,\exp\left(-\tfrac{1}{2}x^2\right) = -\,2x\,.\,\tfrac{1}{2}\exp\left(-\tfrac{1}{2}x^2\right) = x\,.\,\exp\left(-\tfrac{1}{2}x^2\right) = xy.$$

I suggest that the same elementary procedure might be followed with Arts students whose memories of the calculus has become a little rusty. The minus in eqn. (2) is needed to make y a maximum when $x = 0$.

[3] From elementary algebra the student will doubtless recall that the expression $b_1^2 - 4b_0b_2$ is termed the 'discriminant', and is ordinarily used to classify quadratic equations according to the type of roots that may occur. The use of a ratio instead of a difference enables us to include as special cases those in which one or other of the constants is zero.

(*A*) (i) If k is negative, the roots of the quadratic are real and of opposite sign. The curve which then results is called by Pearson Type I. It is skew and of limited range.

(ii) If k is positive and greater than unity, the roots are real and of the same sign. The curve is called Type VI. It is limited in one direction and unlimited in the others.

(iii) If k is positive and less than unity, the roots are conjugate imaginaries. The curve is called Type IV. It is of unlimited range in both directions.

These are the three main types.

(*B*) (iv) If k is unity, then $b_1^2 - 4b_0b_2 = 0$, and the roots are real and equal. This yields a rather uncommon transitional type, known as Type V, with the range limited in one direction only.

(v) If k is zero, the curve will be symmetrical. It may take three forms—normal (range unlimited), leptokurtic (range unlimited,) or platykurtic (range limited).

(vi) If k is infinite, we obtain a curve known as Type III, limited in one direction usually bell-shaped, but sometimes J-shaped.

(i) In eqn. (4) the two factors differ primarily in sign. By making certain obvious substitutions we can rewrite eqn. (2) as follows:

$$\frac{d(\log y)}{dx} = \frac{1}{y}\frac{dy}{dx} = \frac{(n_1+n_2)\,x + (n_2A_1 - n_1A_2)}{(x+A_1)\,(x-A_2)} = \frac{n_1}{x+A_1} + \frac{n_2}{x-A_2}.$$

Integrating we then get

$$\log y = n_1 \log\,(x+A_1) + n_2 \log\,(x-A_2)$$

or

$$y = c(x+A_1)^{n_1}\,(x-A_2)^{n_2}. \tag{6}$$

On transferring the origin to the mode, we have

$$y = y_0\left(1+\frac{x}{a_1}\right)^{m_1}\left(1-\frac{x}{a_2}\right)^{m^2}. \tag{7}$$

This is the formula commonly given for a frequency curve of Type I. It is the type of curve that is most appropriate for describing frequency distributions obtained with mental tests where the range is limited.

The student will most readily understand the consequences if we put y_0, a_1, a_2, m_1, and m_2 all equal to 1. Then

$$y = (1+x)\,(1-x) = 1 - x^2. \tag{8}$$

The curve is obviously symmetrical: $y=0$ when $x=$ either $+1$ or -1; and $y=1$ when $x=0$, i.e. at the average. As will easily be seen on plotting the curve, it is in fact a parabola, with its vertex pointing upwards and its limbs cutting the base-line quite sharply at either extremity—a bowler hat rather than a cocked hat. Now put $m_1 = m_2 = 2$. The tails of the curve become concave, though the greater part is still convex upwards. As the indexes $m_1 = m_2$ are increased, the extremities become more and more concave, and the likeness to the cocked hat appears. In this case we can write eqn. (7) in the form

$$y = \left(1 - \frac{x^2}{a^2}\right)^m. \tag{9}$$

The curve then obtained (a transition type), belongs to Type II; there is another form of Type II—U-shaped—which arises when m is negative; Type VII arises when m is negative and the minus sign of eqn. (9) is changed to plus.

Now, as the student with an elementary knowledge of algebra will know, when a and m are both increased indefinitely, we have in the limit

$$y = \left(1 - \frac{x^2}{m}\right)^m = \exp\,(-x^2), \tag{10}$$

and here once again we reach the formula for the normal curve.

We thus see that the equation for the hypergeometrical distribution is simply a more generalized version of the equation for the normal distribution. By making a_1 and a_2 different, we can make the curve asymmetrical; and by keeping m_1 and m_2 finite, we can give it a limited range instead of an infinite range.

(ii) If the roots are of the same sign, the formula becomes

$$y = y_0\left(1 - \frac{x}{a_1}\right)^{m_1}\left(1 - \frac{x}{a_2}\right)^{m_2},$$

which for purposes of calculation can be more conveniently expressed by putting the origin at the start of the curve, which gives

$$y = y_0(x-a)^{m_1}\, x^{-m_2}. \tag{11}$$

The curve thus obtained (Type VI) is limited in one direction only; and the range is from a to ∞. It fits data obtained from measurements expressed in terms of time.

(iii) Now consider the case in which the quantities A_1 and A_2 are not real but imaginary. Assuming first of all that they are equal, we can write $A_1 = A_2 = \sqrt{(-1)}A$. Then we can put our initial eqn. (2) in the form

$$\frac{d(\log y)}{dx} = \frac{1}{y}\frac{dy}{dx} = \frac{X+c}{b_2(X^2+A^2)}. \tag{12}$$

The integration turns on the fact that

$$\frac{dx}{A^2+X^2} = \frac{1}{A}\tan^{-1}\frac{X}{A} + c\ ;$$

and the final result is

$$y = y_0\left(1 + \frac{x^2}{a^2}\right)^{-m} \exp.\,(-\nu\tan^{-1} x/a). \tag{13}$$

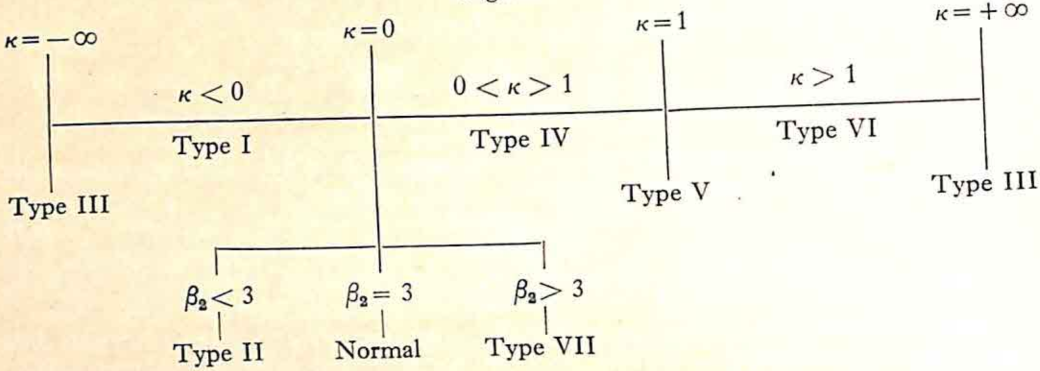
This is the formula commonly given for a frequency curve of Type IV. The curve is not only skew, but is of unlimited range in both directions. It is the type of curve used by Burt to express the distribution of intelligence.

Here it is unnecessary to discuss the remaining types in any detail. I will merely observe that Type III expresses the sampling distribution of variances and of *chi*-squared, as well as the curve for the skew-binomial.

The beta-coefficients to which Dr. Isaacs refers are the ratios of certain powers of the moments. β_1 is a measure of skewness; when $\beta_1 = 0$, the curve is symmetrical. β_2 is a measure of kurtosis; when $\beta_2 = 3$, the curve is normal; when β_2 is less than 3, the curve is said to be platykurtic, i.e. flat-topped; when β_2 is greater than 3, the curve is said to be leptokurtic, i.e. it is more peaked than the normal curve and has longer tails. The terms are due to Karl Pearson (*Biometrika*, IV, 1905–6, pp. 169f.). As Yule and Kendall point out, Pearson by a slip applies leptokurtosis to the distributions for which β_2 is less than 3 (*Introduction to the Theory of Statistics*, p. 165). Aitken has apparently followed Pearson. Kelly, Elderton, and most other writers tacitly make the obvious correction.

If the student will take two normally distributed samples—one large and one small—the smaller sample having a lower mean and a larger deviation than the larger, and then add the frequencies, he will find the resultant curve can be fitted moderately well by means of the formula for Type IV. Evidently such a curve will be (i) skew, (ii) peaked at the mode, and (iii) long-tailed in either direction, i.e. leptokurtic. If a series of such normal curves are superposed, the differences between them being graded, then the composite distribution tends to be algebraically identical with that of Type IV. This represents the frequency distribution of a graded characteristic resulting from both polygenes and major genes when there is no very clear-cut distinction between the two. It will be seen that a curve of this type conforms with the specifications deduced from the model described above.

Figure 1



Practical Implications. Dr. Isaacs inquires why, since the curves seem so similar when plotted, should so much stress be laid upon the disparities? The answer is that, although the graphs may look much the same, the actual figures, when computed for large populations, differ considerably. The practical importance of the differences became very clear when the London County Council was considering the special accommodation needed for pupils toward either end of the intellectual scale—the subnormal and the supernormal.

When the Council took over the organization of London schools from the old London School Board, one of the problems which aroused prolonged consideration and discussion was the kind of educational provision to be made for the abler pupils. The officials who planned the new scheme—the Education Officer, the Chief Inspector, and their colleagues —were all psychologically minded and avowed disciples of the Galton-Sully school of thought. By the age of 10 or 11, they argued, there were many pupils whose educational capacity was so far above the average that they needed to be doing the work of a class that was *more* than one class ahead of that which would be normal for their actual age: (Measured by a ' quotient ' their abilities would be 115 per cent of the average or over). For these a special type of school seemed needed. Accordingly from 1911 onwards the Council organized ' Central Schools ' (taking the place of the old ' Higher Grade ' Schools). But among the brighter children there was a small proportion of children whose abilities differed by more than twice the amount specified, i.e. whose abilities were more than 130 per cent of the average. In the ordinary school therefore they would be able at the age of 10 to do work of a type carried out by children aged 13; and, since the school leaving age was then 14, they would be marking time for the rest of their school career. It was argued therefore, that such children deserved a ' free place ' in one of the ' secondary schools ' (a phrase which in those days meant schools of a ' grammar ' or ' technical ' type), together with a maintenance grant if necessary. To meet their needs a revised scheme was introduced, in accordance with the provisions of the Education Act of 1918, whereby the children were selected by a special examination and awarded ' junior county scholarships '.[1]

Now let us consider the estimates of the numbers likely to be involved. According to surveys carried out in the Council's schools between 1913 and 1917, the total number of pupils in these two groups amounted to 11½ per cent of the age-group concerned. Of this number about 9 per cent were of ' central school type ' (mental age of 11½ to 13 years at the age of 10) and about 2½ per cent of ' scholarship type ' (mental age of 13 or upwards).

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[1] Actually a scholarship system had been tentatively introduced some years before, on a more or less experimental basis, but the detailed history does not concern us here.

Were we to assume a normal distribution of educational ability, the borderline cutting off 11·5 per cent would lie at +1·20 S.D. Twice that deviation would be +2·40 S.D. However this cuts off, not 2·5 per cent but only 0·8 per cent. Thus, *had the estimates been based on the assumption of strict normality, the provision made for the exceptionally bright would have been gravely inadequate.* In keeping with the orthodox belief that the distribution was symmetrical, we find similar arguments being used in the case of the borderlines for the 'dull and backward' (I.Q. 85–70) and the 'certifiable defective' (I.Q. 70–50); and once again the *actual* numbers of the more extreme cases were found to be far greater than the assumption of normality would have suggested.

Nor is the problem merely one of ancient history. It is strictly relevant to the tangled problem of selection at 11 plus. Quite recently (March 2, 1962) the Minister of Education was asked in Parliament about the handicaps from which (it was alleged) the most highly gifted children suffered during their school years, and an inquiry was made about their probable number. In reply he quoted estimates stating that there were only "one or two in a million with I.Q.'s over 175". Assuming in accordance with the usual convention that the standard deviation is 15 I.Q., then the borderline mentioned would be equivalent to 5 S.D., and were the distribution strictly normal, the estimate cited would be perfectly correct. However, the statement at once elicited a number of protests from headmasters of various preparatory and private schools who claimed that their own experience showed the theoretical figure suggested was a gross underestimate; and I for one would readily endorse their objections.[1]

The main differences between curves of Type IV and the normal curve arise, as we have seen, from the elongated tails of the former. And the point which our critics overlook is this: both teachers and educationalists are very apt to underrate the deficiency of the extremely defective and the high intelligence of the extremely able. This wide dispersion was the feature that struck Galton at the very outset of his work. He gives a striking example in the opening chapters of his book on *Hereditary Genius*. There he prints a table of the marks obtained by candidates who were awarded honours in mathematics at Cambridge. Slightly condensed it as follows:

	No. of candidates
Up to 2000 marks	157
2000 to 4000 marks	35
4000 to 6000 marks	7
Nearly 8000 marks	1
Total	200

The mean mark of an honours candidate is approximately 1000. But the Senior Wrangler obtains nearly 8 times that number! The next best candidate falls below him by over 2000 marks. Note that while the average difference between any two ordinary honours men is less than 10 marks, the gap between the two top candidates is 200 times that amount. Those who deal with the mentally deficient have pointed out that there are equally wide differences at the opposite end of the intellectual range. It is these extreme divergences that have led people in the past to think of geniuses and of idiots or imbeciles as freaks or even as forming a different variety or species.[2]

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[1] See, for example, *The Times Educational Supplement* for March 9, (p. 463) and March 16 (p. 517).

[2] F. Galton, *Hereditary Genius*, p. 19. In the text Galton also quotes marks awarded to the top two or three candidates in other years where the senior wranglers obtained from two to ten times the marks of the second wrangler. He adds that, on discussing the matter with the tutors and examiners themselves, they assured him that, if anything, the marks underestimated the gap that separated these rare prodigies from the next best men.

Dr. Isaacs describes both Galton and Pearson as " blind devotees of the normal curve " and " wanting to impose it on everything ", and this is a common allegation. Both, however, recognized its limitations. Galton, for example, explicitly says that " the law of error is incorrect in many groups of vital and social phenomena, although it has been applied to them with partial success and corresponding convenience "; and Pearson more than once emphasizes that " Nature prefers a hypergeometrical to a binomial series."[1]

Social Mobility. Dr. Isaacs goes on to criticize Galton's confident assertion that " few who possess these very high abilities can fail to achieve eminence ", and the assumption (which he attributes to Miss Conway and myself) that " in this country there has always been a tendency for able youngsters to rise in social rank " so that " all ranks of society from the villein to the monarch form a far more united community than the critics of class distinctions would have us believe." Here, however, if I may speak from my rather variegated experience, I should say that the observations of the practical teacher and social worker tend to confirm Galton's view rather than refute it. At times, it is true, (e.g. *Hereditary Genius*, pp. 39 and 49) Galton appears to talk as though the mute inglorious Miltons never existed. But, when we examine the context we find that he also assumes the presence of " power of working " (i.e. health and energy which he holds to be largely innate) and " the eagerness to work ". And he expressly admits that a " few " may fail.

Those who, like Dr. Stott and others[2], complain that " the exponents of individual differences and the advocates of mental testing are seeking to revive the ' religion of inequality ' and delay the breakdown of the rigid class barriers we have inherited from the past " seem to have created (or borrowed from each other) a purely fictitious picture of what that past was really like. It is a picture which teachers (who from their studies of English history and the English novel are fairly familiar with the facts) recognize at once to be grossly distorted.[3]

Class distinctions in this country are, and always have been, very different from those elsewhere. A nobility in the continental sense has never existed over here. Britain is an island. Standing armies have therefore been unnecessary. And this of itself has made rank and class far less rigid than in other European countries. The prevailing rule of primogeniture usually forced every son except the eldest to make his own way or else sink in the social scale. The early development of the professions, of trade, and later on what is popularly called the industrial revolution, offered ready opportunities to those who possessed both ability and energy. Owing to the growing demand for labour many a villein who escaped to the town worked his way up: if he eluded capture for a year and a day he became legally free. The professions have always drawn freely from almost every social level.

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[1] F. Galton, *Proc. Roy. Soc.*, XXIX, 1879, p. 365; K. Pearson, *Life and Letters of Francis Galton*, II, p. 340; cf. *Biometrika*, IV, pp. 193f. It was largely because it brought out this extraordinary spacing towards the top and bottom of the intellectual scale that Galton liked to express frequency-distributions by an ogival curve rather than the more usual cocked hat.

[2] Dr. Halsey and Dr. Floud, for example, in their recent criticism of Dr. Warburton " take the view that . . . the history of social mobility in this country excludes the possibility that the distribution of ' intelligence A ' " (i.e. innate general ability) " between social classes is such as to give mean differences between them of the order given by studies of measured intelligence." (J. E. Floud and A. H. Halsey, ' Measured in Intelligence Largely an Acquired Characteristic ', *Brit. J. Educ. Psychol.*, XXVIII, p. 290). But, although they have been more than once invited to do so, they give no indication of the facts of ' history ' which they have in mind.

[3] The picture seems to be derived chiefly from American and continental writers who after all are not likely to be very sound authorities on British social history, and partly perhaps from journalistic statements with a strong political bias or intent.

Villeins could become priests; and a priest might rise to be an archbishop who would rank above all except a royal duke.[1] In later times the most eminent judges and lawyers, and the most skilful physicians often rose from the humblest walks of life. Many monarchs, and in the seventeenth and eighteenth centuries many noble or wealthy patrons, deliberately searched for intelligence and enterprise wherever they could find it, and gave it all their encouragement. And eventually, as Noel Allan has recently pointed out, what he calls the ' intellectual aristocracy ' came into being and formed a fairly select clan or caste, intermarrying amongst themselves—a point of special interest to the geneticist.

An astonishing amount of genealogical data is available which cries out for systematic statistical analysis.[2] Here it is possible only to cite a few well established instances which (with numerous others) entirely disprove the theory of ' rigid class barriers '. It so happens that, a few years ago, a peerage claim made it necessary to trace so far as possible the descendants of the family of Dudley, whose chief members were descended from Henry VII. It was a barony which had fallen into abeyance from 1757 to 1914. It was found that the co-heirs included a game-keeper, a toll-gate keeper, a butcher, a tailor's wife, and a bakers' wife. The ancestors and the descendants of royalty are, of course, those of whom most is known; and the facts are sometimes surprising. Geoffrey Boleyn, the son if a Norfolk villein, came to London somewhere about 1420; and if he did not actually find the city's streets already paved with gold, he soon amassed enough gold to pave his own way. He started as a hatters' apprentice, and then developed a prosperous business of his own; like Dick Whittington from Gloucester, he became Lord Mayor, and married a baron's daughter. His son married an earl's daughter. Their grand-granddaughter was Ann Boleyn, and their great-great-granddaughter Elizabeth I. Thus the proud queen was only fifth in descent from a villein or ' bondsman '. Or, again working backwards, we find that the sixty-four great-great-great-great-grandparents of Queen Elizabeth the Queen Mother include, not only two dukes, three earls, a viscount, a baron, the daughters of a duke, a marquess, an earl, a bishop, and a number of country gentlemen, clergymen, and bankers, but also a London plumber, a London toyseller, and the landlord of the George Inn at Stamford[3].

Dr. Isaacs maintains that, whether we are dealing with families or nations, " we cannot infer from the mere fact of conquest or of successful migration into a new country or a new class that the successful stock enjoyed some inherent superiority ". He holds that it means " little more than substituting one race or racial name for another ": either would have profited just as well from the *Zeitgeist*. And " in countries of mixed race like Britain or the States all the immigrants contribute something to the common culture, and it would be invidious to discriminate."

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[1] Bishop Grosseteste (d. 1253), the greatest of English scholastics, was of lowly birth: (see A. C. Crombie, *Grosseteste and the Origins of Experimental Science*, 1953). Archbishop Laud (d. 1645) was the son of a Reading clothier. William of Wykeham (d. 1404), who became Chancellor of England and founded Winchester School and New College, Oxford, was the son of a husbandman, and proclaimed the unimportance of social origin in his famous motto ' Manners Makyth Man '.

[2] The English parish registers enable us to trace the poorer members of the family much as the records of the Heralds' Visitations and other state records enable us to trace members of high rank. The Oxford records of University graduation go back to the fifteenth century, and have never been adequately investigated. Early studies of the knightage and baronage frequently comment on rise in social rank. Nicholas Upton, a canon of Wells cathedral, relates in his book on heraldry (c. 1440) that " in these days we see how many poor men . . . have become noble, some by their prudence, some by their energy ". Dugdale's *Antiquities of Warwickshire* (1656) and his *Baronage* are full of illuminating data regarding the rise and fall of families, all remarkably well documented. Eyton's *Antiquities of Shropshire* (1861), Barron's *Northamptonshire Families* (1906), Warrands' *Hertfordshire Families* (1907), and W. G. Hoskin's more recent study of the families of a Leicestershire village where the records happen to be unusually detailed, contain further suggestive material for the Galtonian research-worker.

[3] I am indebted to Professor Burt for some of these instances; and he in turn acknowledges his debt more particularly to Sir Anthony Wagner (Garter King of Arms) and to the recent studies published in Wagner's *English Genealogy* (Clarendon Press, 1960).

Let us then test this by actual figures. Dr. Weyl has recently analysed the contribution of different nations to the 'American élite' as represented by *Who's Who in America* (1955–57). After taking into account the relative numbers in the general population he expressed the results in terms of a kind of 'performance quotient' with 100 as the average American contribution. Bringing together the relative figures from his tables and rearranging the order we get the following results:

Nation	*Performance Quotient*
Dutch	172
Lothian Scots	128
Other Scots	126
Jews	123
English	107
Irish	104
German	86
Scandinavian	84
French	80
Spanish	11
Italian	10

The differences are to some extent dependent on social or occupational origin as well as on national origin. Thus in former years the contribution of the English was much higher and of the Irish much lower. English immigrants descended from the clergy make a far larger contribution (162–190) than the rest. The same is true of Jews of rabbinical descent. Moreover Jews who entered America before 1930 made a much poorer showing than those who have entered since the Hitler persecution.[1] Dr. Weyl also points out that certain nations or stocks are more conspicuous for their contributions in some specific spheres than in others. As is generally recognized, Germans are notable contributors to the physical and chemical sciences; Jews to the medical, psychological and social sciences, as well as to music and international finance; negroes to popular entertainment and athletics. Here once again the most natural inference would be that a comparatively slight difference in physique, temperament, special ability, or historical circumstances has given rise to fairly well marked differences in tradition or contemporary culture.

Ability and Income. The concluding part of Dr. Isaacs' letter is concerned with the apparent discrepancy between the normal or nearly normal distribution of ability and the asymmetrical distribution of personal income. He apparently forgets that in actual life output and performance depend not only on ability but also on energy (i.e. capacity for work); and that differences in energy are also largely innate. Now it has been plausibly argued that, as determinants of output, the factors of ability and of energy should be multiplied not merely added. If so the resulting distribution of output would be highly asymmetrical; and consequently if earnings were proportional to output, the distribution of earnings would also be highly asymmetrical.[2]

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[1] 'Dynamics of the American Elite', *Mankind Quarterly*, II, 1961, pp. 48f. Dr. Weyl suggests that the superiority of the descendants of the clergy may be the result of " a process of eugenic mating on the part of the scholarly and clerical element over the past seven or eight centuries ". It is instructive to note that several of the anthropologists who have contributed papers on this or allied topics to the same journal are highly critical of what they term the 'environmentalist dogma' and the 'cultural hypothesis', as maintained for example by Margaret Mead and her disciples.

[2] See 'Ability and Income', *Brit. J. Educ. Psychol.* XIII, pp. 96f.

This of course provides no ethical justification for the way income is distributed; that is an issue with which the psychologist is not concerned; nevertheless it certainly goes far towards explaining the distribution. Let me add that the figures which Dr. Isaacs quotes from the first edition of Professor Tawney's book, relate to conditions in 1910; and therefore have little to do with current problems. The last edition appends figures for 1948; there we learn that 72 per cent of the national income was distributed in wages, salaries, and pay of the forces, and only 28 per cent in professional earnings, profits, interest and rents.

Professor Tawney quite rightly points out that " equality possesses more than one meaning ". The kind of inequality that he acknowledges to be irremediable is an " inequality in natural endowments of character and intelligence "; the kind of inequality that he deprecates is inequality in wealth, educational advancement, general culture, and the various privileges and advantages that are supposed to go with differences in social class. He contends that two can and should be kept apart. " To justify inequality of circumstance or opportunity by reference to differences in personal quality," he says, " it would be necessary to show that the differences are *relevant* to the inequalities "; and this, he declares, cannot be done.

His arguments, however, consist chiefly in stating the opposite view in a ludicrously exaggerated shape, such as no one would seriously seek to maintain, and then making fun of his opponents for defending such absurdities. He inveighs, for instance, in strong terms against " an organization of education which still makes higher education inaccessible to the great majority of working-class children—as though such children had, like anthropoid apes, fewer convolutions in their brains than the children of the well-to-do ". This is gross caricature. Those who have investigated individual differences among children have adduced considerable evidence to suggest that some of them probably *do* have fewer cells (not convolutions) in their brains than others, though no one has ever supposed that the divergences are as great as those between human beings and the average anthropoid ape. As any teacher of experience could testify, " the great majority of working class children " are incapable of " higher education "; but the real grounds for believing this are not speculative hypotheses about the anatomy of their brains, they are actual trials. The evidence of the genetic psychologist—statistical rather than psychological—amply confirms the teachers' impression that such lack of capacity is, as a rule, very largely innate.[1] And obviously limitations in ability *are* ' relevant ' to limitations in earning capacity, in educational advancement, and in the degree of general culture of which each individual is capable. Moreover, if the abilities of the majority are low, how can the " level of general culture " be high, as Dr. Isaacs demands. A culture which was " common to us all " would have an extremely humble level, as (for example) the figures which the B.B.C. publishes regarding the numbers of viewers and listeners for the different programmes sufficiently demonstrate—a point recently made by Mrs. A. H. Blackwell.

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[1] No doubt among the minority who *are* capable of higher education not every one actually receives it; and this is partly, though by no means entirely, due to the imperfections of the selection procedure. But that is not a reason for abolishing all selections and testing at 11 plus. The idea that because ' intelligence tests ' do not discover *every* deserving case, therefore intelligence tests must be discarded is rather like arguing that the hospital's laboratory tests should be discarded because they sometimes fail to diagnose the presence or the nature of a definite disease. The demand that there " could and should be a general education for all, as America has endeavoured to show " is, I ventured to think, not an ideal that is likely to get the best out of everyone. May I quote a recent remark of Lord Francis-Williams, formerly the publicity officer for the Labour government, who writes about a current type of ' racial invasion '—the influx of American ideas and ideals: " In the States the good-guy conformist and the rise, not of the meritocracy, but of the ' best adjusted ' (as guaranteed by some overworked undertrained teacher in Junior High), starts way back in the lowest forms of the schools; but it would be a pity if we too made this our educational aim " (*The American Invasion*, 1962).

I do not for one moment suggest that the arguments I have attempted to summarize here are in any way conclusive. But they do, I would submit, provide a strong presumption in favour of a modified Galtonian view. What is needed is not further armchair discussion nor just lively and popular arguments of the kind that used to flow so easily from the late Professor Tawney's pen, but further data and further statistical analysis; and that of course means, as Galton was always urging, further research.

Editorial Note. Dr. Isaac's question in regard to the ' ability of nations ' are discussed in a postscript appended to Professor Burt's paper (see above pp. 42 f.) Professor Burt points out that his reference to the cartoon in *Punch* was not intended to be a " facetious dismissal " of the subject of eugenics. The cartoon was in fact praised by Galton as neatly summing up his own arguments.[1]

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[1] See Galton's letter to Karl Pearson suggesting that the cartoon should be framed and exhibited in the Eugenics Laboratory at a University College *Soirée* in 1908 (*Life and Letters*, IIIA, pp. 335, 375). The drawing was by George du Maurier (the author and illustrator of *Trilby*), not by Sir John Tenniel. It represents an earl in his coronet gazing over a five-barred gate at his prize bull, and saying: " By Jove, you are a fine fellow! " And the bull replies, " So would you have been, my Lord, if they had taken as much pains in choosing your ancestry as you did in choosing mine " (*Punch*, March 20, 1880).

THE THREE DIMENSIONS OF FACTOR ANALYSIS. I

To the Editor, British Journal of Statistical Psychology

SIR,—In dealing with the questions raised by Dr. Gregson and myself about the 'General Aesthetic Factor', both Miss Williams and Professor Burt seem to introduce needless complications. In their recent discussion of the subject (this *Journal*, XIV, pp. 72–8) they begin by distinguishing between conclusions drawn from 'correlating persons' and conclusions drawn from 'correlating tests'. Then towards the close of their reply they refer to qualities or 'values', and seem to imply that these too have their correlations and their factors. So that we are led eventually to think in terms of three dimensions simultaneously.

Now the experiences to which Mr. Gregson and I were referring are, from a psychological standpoint, quite simple and unitary. We look at a Van Gogh, and say: "What a magnificent sunflower"; at a Rembrandt and say: "What a dignified face"; or rather, we don't *say* anything so banal, but just feel one great aesthetic thrill, which those naïve phrases may serve to indicate.

When we are asked to distinguish between 'persons' as observers or judges, and then again between the different pictures serving as 'tests' of their abilities, and finally between the various 'attributes' on which our evaluation is based, I for one, and probably, I suspect, Mr. Gregson also, feel that our attention is being diverted to 'factors' which, if the truth were known, are probably not aesthetic at all. Professor Burt reminds me of the ornithologist in *Punch*, who is asked about a rare type of seagull in the sky, and insists first of all in getting compasses, spirit-level, and measuring tapes, to determine the latitude, longitude, and altitude of the bird; and meanwhile the bird flies away.

If we *must* introduce factorial techniques, can't we be content—as the vast majority of factorists seem to be (except perhaps Dr. Stephenson and Professor Cattell, who after all are Professor Burt's own pupils)—with merely 'correlating tests or traits'? Why not keep to one type of variable at a time? Why in short have *three* dimensions? C. R. PATERSON.

THE THREE DIMENSIONS OF FACTOR ANALYSIS. II

A Reply

Mr. Paterson in his criticisms assumes that the question at issue was a simple problem in general psychology. Had that been the case, we should readily have agreed that the relevant evidence was to be sought along the lines he suggests. But the problems investigated by Miss Williams, Dr. Dewar, and Professor Burt were problems of individual differences and their apparent causes. It was this that necessitated an attack from various aspects or angles by means of factor analysis; and it was these investigations that Mr. Paterson and Dr. Gregson originally cited.

The attempt to study three types of variable simultaneously—three 'dimensions', as Mr. Paterson calls them—is not, as he seems to suppose, a piece of wilful elaboration occurring specifically in the researches he criticizes. In every kind of scientific inquiry three types of variable are inevitably involved. First, there are the objects or entities selected for study: in the case of the physical sciences they will usually be material things; in the case of psychology they will often be persons. Secondly, there are the properties or qualities manifested in the observable behaviour of these objects or entities. The contrast between these two types of variable is a commonplace of traditional logic; it is the familiar contrast of subject and predicate, substance and attribute, concrete individual and abstract characteristic, the Particular and the Universal. But thirdly, there are the investigators or observers, or rather the different observational situations or occasions whether these imply a difference of observer or not.

In the past science has commonly assumed that the scientific observer could be treated as a constant, and that the process of observation had no effect of itself on what was observed and so could be eliminated from the report of the results. But, as both the theory of relativity and the recent development of the quantum theory indicate, the observer and the process of observation even in physics, must—at least in principle—always be regarded as essential constituents in the total situation—constituents that may easily affect the results obtained. In psychology the influence of this third type of variable is still more obvious.

Take one of the earliest and commonest kinds of research in individual psychology. Psychologists are interested in studying the various traits and intellectual abilities displayed by school children; and for this purpose they obtain marks or assessments for (say) the intelligence, memory, verbal capacity, numerical capacity, etc., of a representative sample. At the very outset it became clear that to rely on the judgement of a single teacher might be misleading. Accordingly it was the practice to obtain parallel judgements from two or more teachers; and the study of their mutual consistency or ‘reliability’ formed an essential part of the research from the very start. Sometimes instead of obtaining assessments from teachers the psychologist obtains scores from a sample consisting of several tests. And once again he will apply and re-apply the tests on successive occasions, and calculate reliabilities. At times he may be interested in the changes from one occasion to another; and the purpose of repeating the observations or tests will then be to study how for the departures from mutual consistency show a steady change or trend—attributable perhaps to maturation or to learning.

In the field of aesthetics the same threefold framework recurs. Thus in our investigations at St. Martin's School of Art, Miss Pelling secured a composition in water-colour from each of the examinees, and then asked art-teachers to rate each picture for a number of aesthetic characteristics—composition, drawing, harmonious colouring, realism, impressionism, significant form, etc. Here the examinees (or their pictures) represent the ‘persons’; the aesthetic characteristics form the ‘traits’; and the art-teachers provide the ‘observers’.

Professor Peel and Professor Pickford have followed a somewhat similar plan in their studies of aesthetic types and artistic appreciation (cf. *Brit. J. Psychol.*, XXXV, 1945, pp. 61–9, and *ibid.*, XXXVIII, 1948, pp. 135–41). Here a number of judges (or ‘observers’) assessed a batch of paintings by well-known artists (the ‘persons’) in regard to such traits as ‘naturalism’, ‘technique’, ‘spontaneity’, ‘emotional expression’, ‘harmony of design’, etc.; the correlations can then be averaged for pictures or for judges, and the results factorized.

The simplest way of dealing with the complex data thus obtained is to average the values for one type of variable and calculate correlations for one of the other types. According to the investigator's dominant interest he can thus choose between P–, T–, and O– techniques, i.e., correlating different persons, correlating different traits (or tests for different traits, and correlating different observers or occasions). Thus *for certain purposes* it may be sufficient, as Mr. Paterson suggests, simply to “correlate tests or traits”. But there are other problems for which it is necessary to correlate persons, and others again for which it is necessary to correlate successive observations. Nevertheless, as one of us has argued elsewhere, it is often still more satisfactory to investigate all three sets of possible correlations simultaneously; and this can best be done by a three-way analysis of variance. For a more detailed discussion of the procedure Mr. Paterson may refer to an earlier issue of this *Journal* (VIII, 1955, pp. 103–118), and particularly to the diagram on p. 110, which illustrates schematically three ‘dimensions’ and their possible interactions.

ELIZABETH WILLIAMS
CYRIL BURT

OBITUARY NOTICE

TRUMAN LEE KELLEY

Statistical psychologists, both in Britain and elsewhere, will regret to hear of the death of one whose name has been familiar to almost all of them since their student days. Truman Lee Kelley, one of our early pioneers, was born in Michigan in 1884. He graduated in mathematics at the University of Illinois. And in 1914 he obtained his doctorate from Columbia University with a thesis on *Educational Guidance*. Its theme was the fascinating and highly complex problem which lay at the base of nearly all his professional work throughout his long career—the problem of individual differences in mental ability and the consequent need for scientific assessment and for systematic guidance. In this early research he decided, on the suggestion of Thorndike, to apply to the specific issues with which he was concerned what were then the novel Pearsonian techniques of multiple correlation and regression, which had recently been " tried out with encouraging success for much the same purpose by English investigators, such as Maxwell Garnett, Burt, Brown and Spearman". A little later he became an instructor in educational psychology at the University of Texas; and during the first World War served as psychological consultant for the Committee on Classification of Personnel.

As soon as the war was over he came to University College, London, with a view to studying at first hand the statistical procedures that were being developed and applied by Pearson and his colleagues in the Galton Biometric Laboratory, and to make the acquaintance of British psychologists who were adopting or adapting Galton's methods to Galton's problems. In 1923 he published his work of *Statistical Method*, which remained for long the leading textbook for psychological and educational research students who desired to make use of such techniques. Here he gave the first really lucid and systematic account of Pearson's classification of frequency curves and of his various formulae for different forms of simple and multiple correlation. In compiling the volume, as he generously observes, " the English school has been most contributive "; " the greatest inspiration has been the product of that master analyst, Karl Pearson ". Nevertheless, he himself was able to contribute several original suggestions of permanent value, notably new methods for determining probable errors and for defining and estimating ' reliability '. His chapter on ' sundry problems ' includes several biological applications, such as ' the determination of the cross-over value of a chromosome section '. One of his most useful contributions was the compilation of various tables (rather different from those already existing) for facilitating the use of the normal curve and for aiding the calculation of partial correlations and regressions. These were later amplified and extended in the *Kelley Statistical Tables*, first issued in 1938, and revised and supplemented ten years later.

From 1920 to 1931 Kelley was Professor of Education at Stanford University. He assisted in the construction and standardization of the well-known Stanford Achievement Tests; and shortly afterwards published his second major work—*The Interpretation of Educational Measurements*. But for immediate influence the book entitled *Crossroads in the Mind of Men* was perhaps the most important that he ever wrote. While studying in London, he had taken an active part in the informal debates—sometimes quite heated —on the alternative methods for carrying out what has since become known as factor analysis. His previous book on *Statistical Method* was one of the few publications of that day that included in its references Pearson's important study of ' Lines and Surfaces of Closest Fit ' (1901)—a paper which described the method of principal axes and so formed the starting-point for all factorial techniques. Spearman's first contribution to the subject, insisting on a single factor method, had been published some years before in an American journal, but had aroused little but a passing wave of criticism from Thorndike and his co-workers. In his *Crossroads* Kelley now drew renewed attention to Pearson's earlier multi-dimensional approach; and, by elaborating a ' pentad criterion ' in place of Spearman's ' tetrad criterion ', he was able to show how the Spearman theory was merely a special and highly simplified case of a far more general technique. This marked the beginning of an active American interest in what had previously been a purely British set of technical procedures.

In a shorter work entitled *Essential Traits of Mental Life* (1935) he presented his own working methods for determining the principal axes of the correlational ellipsoid, and included an instructive chapter describing, with examples, a detailed ' comparison of components determined by the principal axes method and by the centre of gravity method ' (which had recently been popularized by Thurstone). Kelley's method, in its general character, is equivalent to Burt's method of ' weighted summation ' (as distinct from his method of ' simple summation ' which Thurstone re-named the ' centroid method '). Kelley's ' new ' working procedure turns out to be (with minor modifications) admirably adapted to work with the electronic computer (it is really a revival of the process of canonical reduction described long ago by Jacobi and Weierstrass). The results, so Kelley maintained, threw serious doubt on the practical value and the logical validity of the simpler procedure. The controversy that followed, like nearly all such controversies, proved particularly fruitful, because of the attention it excited and the mass of investigations that it inspired. In contrast to Pearson, Kelley was entirely ready to accept a Mendelian approach to the problems of human heredity, and to give full credit to the influence of non-genetic factors. " Genetic origins, " he writes, " almost infinitely extensive, interacting with the physiological and the social environment, find their fruition in beings who run more or less true to survival types in essential physical and mental make-up, but yet show great variability in non-essential detail."

From 1931 until his retirement in 1950 he held the chair of Education at the Harvard Graduate School of Education. His last important work, and in some ways his most ambitious, was *The Fundamentals of Statistics*. This has attracted far less notice than it merited, possibly owing to the somewhat unattractive style in which it was printed. Here the emphasis is placed largely on the concept of scientific method—a topic on which he had frequently written and lectured. A sentence from the preface might serve well to sum up his attitude to the whole subject. " The content of statistics is not a bag of mathematical tricks, but a set of rational thought processes designed to aid the solution of problem situations in every walk of life; there is in these processes a nicety which gives zest to their discovery and pleasure in their use."

BOOK REVIEWS

Mathematical Thinking in the Measurement of Behaviour. Edited by HERBERT SOLOMON. Illinois : The Free Press of Glencoe, 1960. Pp. 314. $7.50.

The University of Columbia enjoys a long-standing reputation for pioneer work in studies which apply mathematical techniques to problems in psychology and other social sciences; and in its Bureau of Applied Social Research has recently inaugurated an elaborate programme of investigations, some of which are sponsored by the Office of Naval Research. The first publication to emerge as a result of the scheme was the admirable work on *Games and Decisions* by Duncan Luce and Howard Raiffa—a volume which is, or should be, on the shelves of every psychological library. The present volume is the second in the series. As with the other surveys the chief emphasis is on the mathematical concepts and techniques developed to deal with particular types of problem; and no attempt is made to review the results obtained or to develop an up-to-date theory of the subject-matter concerned.

The first of the three surveys in the present book (by Professor J. S. Coleman, now of John Hopkins University) deals with ' the mathematical study of small groups '. It consists of an intensive examination of four types of model: (i) models which describe the joint implications of qualitative relations, like those of H. A. Simon; (ii) various interaction models in small discussion groups, like those of Bales, Stephan, and Keller; (iii) relational models, like the random choice models of Rapoport, Leeman's sociometric choice model, and the dominance schemes used by Rapoport, Landau, and others in the attempt to develop a model for describing status hierarchies; and finally (iv) models of group action, such as the Hays-Bush stochastic model of group learning and the Lorge-Solomon group problem-solving model. Each is formulated and discussed with unusual clarity; and the whole survey ends with a critical summary and evaluation.

The second survey, by Professor E. W. Adams, is in many ways the most illuminating. It is concerned with what the writer calls the ' Bernoullian Utility Theory '. This is another name for what Duncan Luce terms the ' linear utility theory ', and Ward Edwards ' expected utility maximization theory '.[1] Current interest in the theory dates from Neumann and Morgenstern's classic *Theory of Games and Economic Behaviour* (1947). The term ' utility ' was introduced by the British Utilitarian school to designate in the economic field what psychologists and moralists had termed in a wider sphere ' value '. The kind of situation envisaged is that in which a purchaser considers whether or not he shall buy some commodity—say a typewriting machine; in coming to a decision he has to weigh the probable worth to him of the machine when purchased against the known or probable price he will have to pay. This leads to a hedonic calculus in which the pleasure or satisfaction to be attained by some course of action is balanced against the probable pain, effort, or other sacrifice of pleasure which the action will entail. The term ' utility ' thus denotes the relative desirability to me of the object at which I decide to aim—i.e., the comparative strength of my preference for it. Bernoulli's fundamental hypothesis was that the utility of an uncertain alternative may be measured by the statistically *expected value* of the various utilities of its possible outcomes.

In all this, as Professor Adams points out, three essential concepts are involved: (i) a set of alternatives among which the individual must make a *choice*; (ii) the system of values or *preferences* by which he ranks the alternatives; (iii) the *decision* which he finally makes. Each of these concepts is manifestly subjective, and imply that we think of the person concerned as conscious agent. It is a curious comment on the dominance of behaviouristic taboos that Professor Adams finds it necessary to apologize for introducing such improper terms. He seeks to pacify potential critics by assuring them that the analysis adopted " simply functions as a heuristic guide to the construction of theories whose scientific meanings lie entirely in their observable consequences ".

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[1] Over two hundred years ago Daniel Bernoulli deduced the Weber–Fechner formula from theoretical considerations regarding motivation, and this presumably is the reason for the present designation: (Specimen theoriae novae de mesura sortis, *Com. Acad. Scient. Imp.*, Petersburg, 1738, pp. 177–182). Laplace and Fechner both took up the notion. But the intellectualist outlook of psychology during the nineteenth century left this aspect of their work undeveloped. (See this *Journal*, XIII, p. 5, and L. J. Savage's *Foundations of Statistics*, 1954.)

Professor Adams then proceeds to discuss in turn the scaling problems thus involved, the appropriate methods for determining the amount of risk, the various assumptions implied by the theory and their observable consequences, and finally the different solutions proposed for the decision problems. All these are formulated in symbolic terms, and testable inferences algebraically deduced. Throughout the discussion the author has, as he says, attempted to keep the mathematical technicalities down to a minimum; but he assumes that the reader will be familiar with such notions as " class membership, elementary set operations, and the relevant functions and relations ", as well as the standard notation for such concepts.

The survey concludes with a brief discussion of empirical applications of the Bernoullian utility theory. Although the theory was first revived and revised with a view to its use in the sphere of economics, it has since been largely transplanted to the field of psychology. This is no doubt partly because contemporary economists have tended increasingly to drop the older attempts to interpret mass behaviour in terms of individualistic processes, and partly because economic phenomena cannot readily be controlled for purposes of experimental investigations, whereas in psychology it is quite practicable to test tentative hypotheses by devising artificially controlled situations.[1]

Accordingly, to illustrate how the theory has been applied, Professor Adams selects only one investigation from economics, but two from psychology. It is, however, the first of these that most clearly reveals the weaknesses of the theory. This is a study by Friedman and Savage to explain why people gamble. In the most typical forms of gambling there is generally a very small probability of winning a large amount and a very large probability of losing a small amount. It was this kind of problem which led Bernoulli to put forward his ' utility theory '; his suggestion was that the value of money gained by an individual is proportional not to its absolute amount, but to its ratio to what the individual already possesses. But, of course, the majority of gamblers do not begin by making rational estimates of their probable expectations; they are influenced primarily by the excitement of participating in a possible if somewhat imaginary stroke of luck—a purely emotional factor which is deliberately left out in the mathematical theory. This necessarily makes the whole argument somewhat academic; and, as Professor Adams rightly concludes, the chief lesson to be learnt from a study of actual researches is the great difficulty of designing a significant experiment to test a set of hypotheses which at first sight look so attractively simple.

The last of the papers included in this volume—' A Survey of Mathematical Models in Factor Analysis ' by Herbert Solomon, now of Stanford University—is the shortest of the three, and the one which will be of greatest interest to British students of statistical psychology. The mathematical treatment in the main is excellent; but the historical framework of the discussion is sadly at fault. Factor analysis, as he rightly says, began with Karl Pearson's paper on ' Lines of Closest Fit ', which applied the method of least squares to solve " problems faced by Scotland Yard in the classification of criminals: . . . its appearance was as a research technique rather than a mathematization of a psychological model. At that time ", he continues, " Galton was developing his ideas on correlation . . . and also suggested the method of transformation known as ' simple ' or ' unweighted summation ' ". Galton's paper on correlation, however, was published in 1888; Pearson's paper not till 1901. Fitting the ' closest lines ' (or factors) by the method of least squares would have involved a method of ' weighted summation '; the method of ' simple ' or ' unweighted summation ' (together with both the designations) were introduced by Burt.

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[1] It is perhaps worth noting that Professor Edgeworth eighty years ago suggested that the mathematical methods adopted by utilitarian economists might be adapted for the study of motivational problems in psychology: see his tract on *Mathematical Psychics: An Essay in the Application of Mathematics to the Moral Sciences*, London School of Economics Reprints, 1932.

" In the form in which factor analysis is now most frequently encountered ", we are told, " its development began with a pioneering paper by Spearman (1904) ". The children in a village school and a preparatory school for boys " were given four achievement tests and two sensory discrimination tests . . . The test data were presented in the form of a correlation matrix . . . which led to the birth of factor analysis. The correlations were based on test scores for 36 boys." After examining the matrix, " Spearman perceived a functional relationship, namely, the ratio of any corresponding pair of elements in any two columns was constant . . . $r_{ac}/r_{ad}=r_{bc}/r_{bd}$ ". " The fact that an $n \times n$ matrix would have rank one simply by chance would be regarded with suspicion "; and this led Spearman to conjecture that the response to a test was " made up of a general intellective factor and a factor specific to the test ". Thomson, however, held that, " though the data were not inconsistent with the notion of a common element, neither were they inconsistent with its non-existence ".

There are here numerous misunderstandings. The correlation table which Professor Solomon reproduces was obtained from the boys at the preparatory school only: (Latin and French are not taught in a ' village school '). Only 22 boys, not 36, were available for all the assessments. Only one test of sensory discrimination was applied; and no ' tests of achievement ' were given: the assessments for Latin, French, and other school subjects consisted of teachers' reports on the boys' ' place in class '. Spearman never regarded his correlation tables as ' matrices ', and indeed deprecated the use of matrix terminology and matrix algebra. Nor does his early paper include any techniques resembling so-called factor analysis; the word ' factor ' is used only with reference to such causal influences as sex, age, and the like. Spearman's procedure indeed is an anticipation of what was later termed ' canonical correlation ' rather than of ' factor analysis '. The ratio criterion for a matrix of rank one—$r_{ac}/r_{ad}=r_{bc}/r_{bd}$—nowhere appears; this was first used by Burt in his 1909 paper. Spearman at this stage regarded the hierarchy as a matter of rank-order, not of ratios. The notion of a general intellective factor or ' general ability ', as he termed it, was due to Galton, who also postulated the presence of ' special abilities ', i.e. group factors; and, as Thomson points out, " it is the *absence of group factors* which is the crux of Spearman's theory ", not the possible presence of a general factor.[1] These misstatements are extremely common in summaries of the early history of factor analysis written by American pschologists; presumably the British papers to which they refer are not readily accessible, and the writers therefore borrow the descriptions from each other without verifying their accuracy.

To illustrate what he takes to be Thomson's view, Solomon constructs an artificial set of four overlapping ' bonds ' (or ' group factors ') which yield a correlation matrix having a rank of one. This shows, he contends, that the ' Spearman model ' and the ' Thomson model ' are alternative ways of interpreting the same set of correlational data. In point of fact what it shows is more than this. To obtain a matrix of rank one there has to be a certain *system* underlying the chosen arrangement of bonds. The several tests consist of 5, 4, 3, and 2 ' elements ' respectively. Hence the saturation denominators of the saturation coefficients will be $\sqrt{5}$, $\sqrt{4}$, $\sqrt{3}$, and $\sqrt{2}$ respectively. These elements must overlap in such a way that the correlations that result have a rank of one: in the present case four elements must be shared by both tests 1 and 2, two must be shared by test 1 in common with tests 3 and 4. And this yields a general factor with a saturation of $2/\sqrt{5}$, $2/\sqrt{4}$, $1/\sqrt{3}$, and $1/\sqrt{2}$ respectively. Thus the general factor provides a mathematical formulation for the system which has guided the arrangement of the ' elements '.

Professor Solomon next turns to Guttman's analysis, arguing that " its contribution and relevance are not diminished by any grandiose claims the author may have for it ". This section is admirable; and well deserves the attention of British factorists, who have hitherto tended to neglect the ingenious procedures devised by Guttman. The whole discussion concludes with a brief account of the procedures proposed by Thurstone, Holzinger, and Hotelling, respectively.

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[1] W. Brown and G. Thomson, *The Essentials of Mental Measurement*, 1925, p. 189.

The most serious defect of the survey is the omission of any reference to the type of factor analysis which has been chiefly favoured by British writers, namely, that which was envisaged from the very outset by Galton himself. This assumes that the factors entering into a set of cognitive tests consist of *both* a general cognitive factor *and* several more or less specialized group factors, often overlapping. Spearman denied the group factors; Thomson (at least as Solomon interprets him) denied the general factor. Later experience has shown that, more often than not, both types are present. The Holzinger model does not really meet this situation, since (as Solomon points out) with that model " any test having one group factor cannot have another group factor ". In addition, he says, the Holzinger model entails " a problem not easily resolved ", namely, the way in which the tests are to be classified. In British investigations the classification and subclassification into groups is based on the preliminary bipolar analysis. All this has, of course, been frequently stated in papers published in this journal as well as in earlier publications going back over forty years or more; but it still seems unfamiliar to those American writers who attempt a historical review of factorial procedures.

W. R. ROGER

Developments in Mathematical Psychology. Edited by R. D. LUCE. Illinois : The Free Press of Glencoe, 1960. Pp. x + 294. $10.

This is the third of the volumes issued by the Behavioral Models Project of the Bureau of Applied Social Research, Columbia University. It consists of three contributions only; and consequently, as the preface confesses, does not by any means cover the whole field suggested by the title.

The first section, contributed by the editor, is by far the best of the three. It deals with 'the theory of selective information and some of its behavioral applications'. It starts with an admirable introduction to the theory of information, so far as it has been applied to problems of psychology, and then reviews the more significant investigations in which information theory has been used. The second section by R. R. Bush, presents ' a survey of mathematical learning theory '. It is confined mainly to an account of the work on learning carried out by Estes, Burke, and Bush and Mosteller. In the last section J. C. R. Licklider discusses ' quasi-linear operator models in the study of manual tracking '. He gives an instructive account of the techniques employed in signal analysis, and examines the attempts made to describe certain types of human activity in terms of the concepts adopted to explain the behaviour of servo-mechanisms.

The book as a whole is perhaps less satisfying than the two earlier volumes in the same series. But it provides a useful review of the three rather specialized fields of research with which it sets out to deal.

W. R. ROGER

Quantitative Methods in Psychology. By DON LEWIS. New York : McGraw-Hill Book Company, 1960. Pp. xii + 558.

This is a revised and enlarged edition of a textbook prepared by Professor Lewis some fifteen years ago. From personal experience he had reached a conclusion, which will be shared by many teachers of psychology in this country, that " graduate students, with or without college training in mathematics, usually have not formed habits of thinking clearly in quantitative terms, and do not acquire such habits until they have been tutored extensively in the use of mathematics as a tool of scientific inquiry ". Professor Lewis's aim has been to supply such a tutorial course.

The book begins with an explanation of the fundamental concepts of mathematics—'variable', 'function', and the like—so far as they are relevant to the work of the psychologist. The student is then familiarized with the chief methods of fitting straight lines. A discussion of logarithms, and their theoretical and practical uses, leads to a most useful account of the fitting of non-linear functions. After this we have a couple of chapters stating and illustrating the basic rules for differentiation and integration; and these are then used to derive formulae for the more important types of statistical distribution—the normal curve, the Poisson distribution, the distribution of chi-squared, and so on. The next chapter deals in considerable detail with tests of significance, of goodness of fit, and of linearity, and then introduces the student to the analysis of variance. The last chapter is perhaps the most interesting of all. It presents a detailed discussion, more or less in historical order, of the various applications of mathematical techniques to psychological problems carried out by such pioneers as Thurstone, Clarke Hull, Stevens, and Estes. To each chapter in turn a number of well chosen exercises are appended.

Considering the size of the volume, the ground covered both as regards mathematical theory and statistical procedures will seem to many somewhat limited. A number of highly relevant topics are omitted. Unfortunately too several time-honoured errors and misconceptions, fairly common in textbooks on statistical psychology twenty or thirty years ago, are still allowed to remain in the present edition. For this and other reasons it is scarcely a book which British teachers are likely to place into the hands of their students. But the teachers themselves will find it both helpful and suggestive. P. G. PHILLIPS

The Growth of Basic Mathematical and Scientific Concepts in Children.

By K. LOVELL. London: University of London Press, 1961. Pp. 154. 15*s*.

The purpose of this book is to describe the stages by which the average child gradually acquires his final notions about such things as substance, space, time, and number. In the main Dr. Lovell has followed the methods and the theories of Professor Piaget; and Piaget's chief collaborator in this field, Professor Bärbel Inhelder, contributes a brief foreword. Occasionally, however, both in his techniques and in his conclusions Dr. Lovell departs from those adopted by the Swiss investigators; and, where he does so, it is always, I venture to think, a definite improvement.

The greater part of the volume consists of abridged accounts of Dr. Lovell's ingenious experiments—already reported in more technical detail in various educational journals. These are supplemented in each case by a simplified discussion of the various considerations which led up to the investigation. Professor Piaget and his colleagues tested and observed a comparatively small number of children and preferred a flexible method of experimentation, because, as they say, "when we were exploring ideas which are as yet unknown to us, it seemed essential to proceed in the most adaptable fashion". This, however, often leads the sceptical reader to wonder how far the results thus obtained are really typical. Dr. Lovell, on the other hand, has used a standardized procedure, and subjected his data to statistical checks.

After briefly describing the processes of concept formation, he discusses at some length the logical bases of mathematics in general and of the natural numbers in particular. This leads to two chapters on the best mode of approaching number in the class room, with a series of useful suggestions for the practical teacher. 'Substance' he defines, in the rather restricted sense, as 'quantity of matter', i.e. 'mass'. 'Weight' is defined as 'the quantity of a portion of matter as measured by the amount of its downward force due to gravity' (this he attributes to the *Shorter Oxford Dictionary*; but, as the critical reader will easily guess, the quotation is not quite correct). Experiments in junior schools, dealing with both these concepts, are described and discussed in some detail; and the chief conclusion is that " the development of such concepts is much more complex than Piaget reckons ". We then proceed in turn to time, space, length and measurement, area and volume, coming back towards the close to the number system. The last chapter consists of ' final comments'. The main conclusions apparently are that it should be the aim of the teacher " to ensure that the concepts discussed *do* in fact develop ", and, if possible, " to speed up the process ". As yet, however, " we do not know how best to realize these aims ".

As Dr. Lovell rightly insists, it is highly desirable that teachers themselves should attempt experiments and observations along these lines; and his own researches present admirable models which the practical teacher can adapt for his own particular purposes. In so doing, I would venture to suggest that they might well give special attention to the problem of individual differences. All the investigators—Swiss as well as English—tend at times to speak as though there was only one line of development which every boy and girl followed, and constantly talk of ' *the* child '. This perhaps is natural and even necessary in pioneer inquiries which are concerned primarily with sketching the main outlines. In his opening chapter Dr. Lovell himself observes that " it seems that different children arrive at the same concept in different ways: often, however, the result is a different type of concept ", This presents an urgent and more complicated set of problems on which teachers could undoubtedly throw a flood of light. How far, for example, does the approach of the bright differ from that of the dull? How do the procedures adopted (say) by the visualizer differ from those of the child who relies mainly on kinaesthetic or on verbal imagery? It has been stated that those eminent scientists who picture the material universe primarily in terms of points located in three-dimensional space are generally endowed with a clear visual imagination, whilst those who interpret it in terms of force or energy are often non-visualizers with imagery derived mainly from the so-called muscle sense. Do children's concepts vary in this way?

I would suggest too that, in a work which claims to be ' a comprehensive review of present knowledge ' about its subject, a little more attention might be paid to the researches and theories of British psychologists who have approached the same problems along rather different lines. The references Dr. Lovell appends to each of his chapters consist for the most part of authors who have followed lines similar to those of Piaget and his school. In one who has trained at the London Institute of Education it is surprising to find no mention of three former members of its staff who (with their own research students) did so much work in this field—Sir Percy Nunn, Professor H. R. Hamley, and Dr. Susan Isaacs. Nor does he refer to the experimental studies of Miss Wheeler, the factorial studies of Barakat and others, or the numerous investigations of French and German psychologists from Binet and Meumann onwards.

These, however, are minor defects. The book itself is certainly one which all teachers and students in training should read and study.

CYRIL BURT

Mental and Scholastic Tests. By CYRIL BURT. Fourth Edition (Revised). London: Staple Press. Pp. xxx + 551. 65*s*.

There can be few books on psychological topics, published over forty years ago, which, after six or seven reprintings, are still sufficiently in demand to warrant being set up afresh and re-issued. The studies which form the basis of this volume were undertaken when mental and scholastic testing was still in its infancy. The author was the first psychologist to be appointed to an official position as consultant to an education authority; and the educational area covered by that authority was the largest in the world. Many of the new and ingenious devices introduced in the course of these researches have since given rise to a long series of developments in statistical psychology. Here we encounter the first survey of an entire school population, the first attempts at absolute scaling, the first assessments by item analysis, the first demonstration that the distribution of intelligence is approximately normal and yet significantly non-normal, and some of the first applications of factor analysis using what has since become known as the 'centroid' method.

But the book is much more than a historical landmark. For teachers, educationists, educational psychologists, and psychological students generally, it provides an admirable introduction to the techniques of which it treats, and a model for further researches. Where the content of the test problems has become somewhat antiquated, new problems have now been substituted. Fresh specimens for the norms for children's drawings at various ages have been obtained by a new investigation. A good deal of material that would today seem irrelevant owing to altered conditions in the schools has been omitted. And the whole volume has been revised and brought up to date.

In resetting the pages a new and more attractive type-face has been adopted; and the production of the whole book greatly improved. One or two criticisms are perhaps called for. The printer has apparently changed the method used in reproducing the children's drawings. In the original edition the appearance of actual pencil work was skilfully preserved; in the new this effect has unfortunately been lost. It seems a pity too that Professor Burt did not include the age-assignments for the London Revisions of the Stanford-Binet and Terman-Merrill scales. May we hope that when another reprinting is called for, these minor shortcomings will be removed? In all other respects both publishers and authors deserve the warmest congratulations from teachers and psychologists alike.

E. R. JENNINGS

Incentive. By FRANK A. LOGAN. New Haven: Yale University Press, 1960; London: Oxford University Press. Pp. xvi + 285. 48s. net.

From the subtitle the intending reader gathers that the aim of this book is to describe "how the conditions of reinforcement affect the performance of rats". Dr. Logan is Associate Professor in Yale University. The book itself reports the results of five years' cooperative research; and in the light of the data obtained attempts a further development of behaviour theory.

More than a thousand rats were trained to run down a short alley, and over a hundred different conditions of reinforcement were used. It was found that, if the reward remained constant for all the trials in a given series, the performance improved in speed as the reward increased in amount or the delay in giving the reward was diminished. When the reward varied from trial to trial, then performance also varied in keeping with the variations in the amount or promptness of the reward in the preceding trial. When the reward was made to depend on the speed of the performance, then the rats tended to run with the speed that received the highest reward.

Professor Logan maintains that results oblige us to reject "an equilibrium model based on the simplest macromolar assumption that all behaviours that complete a molar response in the same total time can be aggregated together and treated as indicating the same strength of that response". Instead he puts forward a 'micromolar theory'. This starts out from the familiar stimulus-response hypothesis formulated by Hull and Spence, but "extends it so as to include in the definition of the response the quantitative dimensions of the response", and thus "makes the response side of the S–R bond similar to the stimulus side in having both quantitative and qualitative dimensions conceived of as different events among which generalizations occur and discriminations are formed".

A. K. RUSSELL

(1) **Hidden Channels of the Mind.** By LOUISA E. RHINE. London: Victor Gollancz, 1962. Pp. 292. 25*s.*

(2) **Report on an Enquiry into Spontaneous Cases.** By W. H. SALTER, ROSALIND HEYWOOD, and CELIA GREEN. Proceedings of the Society for Psychical Research, LIII, 1960. Pp. 103–194. 10*s.* 6*d.*

A few years before the foundation of the Society for Psychical Research, Francis Galton embarked on his celebrated inquiries into mental imagery. In the course of his researches he was astonished to find " how many sane and often eminent people own to having had hallucinatory experiences of vision or hearing which exhibited all the vividness of natural reality ". A well-known statesman, travelling abroad, had heard the voice of his mother bidding him farewell at the time of her death; a Fellow of the Royal Society had heard a " postman's knock, followed by the entrance into his study of a man in mediaeval costume, who after a brief time faded and disappeared ". When questioned, Galton says, the replies of such people " usually took a form like this: ' No, no; I've never experienced anything of the kind '; and then, after a pause, ' Well, there certainly was one curious thing . . . ' "

Galton, like a true psychologist, was not particularly interested in determining whether or not such experiences were veridical, but rather in the kind of persons who report them and the types of situation that give occasion to them. His own provisional conclusion was that " the visionary tendency is much commoner among sane people than is generally suspected ". He believes it may possibly be " hereditary in certain families, as among the second-sight seers of Scotland, and in certain races, as that of the Gypsies ". He notes that it occurs more frequently at certain ages, e.g., in very early childhood and late adolescence, and more rarely in the elderly, and that it is appreciably increased by certain conditions, fasting, protracted wakefulness, emotional or nervous states, certain drugs, and—most all, he thinks—by a life of solitude. " It seems also ", he says, " to have manifested itself in certain periods of history and at certain epochs of national life." All this suggests, " not that a faculty ordinarily non-existent is suddenly evoked ", but rather that " a faculty long smothered in secret obtains freedom to express itself ". And he urges that a systematic attempt should be made to collect and analyse such experiences.[1]

An early investigation somewhat along these lines was undertaken shortly after the foundation of the Society for Psychical Research, and published under the title of *Phantasms of the living* by Gurney, Myers, and Podmore in 1886. The spontaneous experiences they described and discussed were of many varieties. Their general conclusion seems to have been that those visions which in their opinion seemed to be veridical were chiefly the product of ' subconscious telepathy '. Little attempt, however, was made to investigate the psychological characteristics or conditions distinctive of such occurrences. In 1889 the First International Congress of Experimental Psychology thought the matter well worth systematic research, and appointed a committee to conduct a ' Census of Hallucinations '. A detailed summary was published some five years later. In many ways the most interesting part of the report is the discussion of the nature and origin of hallucinations generally (irrespective of whether they seem verdical) and their relation to analogous experiences, such as illusions.

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[1] See F. Galton, ' Visions of Sane Persons ' (*Royal Institution Proceedings*, 1881), *Inquiries into Human Faculty* (1883, pp. 119f.), *Memories of My Life* (1908, pp. 273f.). Galton himself was inclined to interpret these experiences in terms of the tentative hypothesis that the individual's consciousness is a fragment or a manifestation of a much wider consciousness—one of many " tidal pools thrown up by an unlimited sea " into which it will ultimately be reabsorbed. The analogy is a familiar one; but my impression is that he reached the notion quite independently. However, as a member and later Fellow of Trinity, he was certainly acquainted with Sidgwick and Myers; and at one time he took part in several séances at the home of Sir William Crookes (see his letters to Darwin of March 28, April 19, and May 26, 1872, *ap.* Pearson's *Life and Letters of Francis Galton*, II, pp. 63–5). Nevertheless, when discussing such problems, he usually preferred to remain non-committal both in conversation and in print (see, for example, his paper on ' The Part of Religion in Human Evolution', *National Review*, August 1894).

The committee found that, out of about 17 000 persons who answered their questionnaire, about one in ten stated that they had experienced a hallucination of some kind or another.

During recent years there seems to have been a steady decline in the number of such cases reported or investigated. Popular interest has nearly always been focused predominantly on the problem of veracity.[1] Most people find it difficult to understand that psychologists are interested first and foremost in conscious processes and conscious experiences as such—the colours we see, for example, how they can be analysed, classified, and whether they can be reduced to mixtures of just three primaries, and not whether the world itself is really coloured. And thus, in accordance with the popular trend, most investigators in this field during the last forty years or so, have concentrated mainly on the endeavour to demonstrate the existence of extrasensory modes of cognition; for this purpose carefully controlled experiments in the laboratory, dealing with extremely simple data such as are susceptible of exact statistical analysis, have been found essential.

However, interest in the more general aspects of the problem seems to be reviving. Within the last eighteen months two fairly lengthy publications have appeared, discussing, on the basis of much fresh material, the whole subject of 'spontaneous experiences'. The first is an 'Analysis of Spontaneous Cases', carried out, with assistance from several co-workers, by Miss Celia Green; the second is a survey by Dr. Louisa Rhine. From several thousand cases collected by the Laboratory of Parapsychology at Duke University, Mrs. Rhine has selected about 230 which she regards as typical. Her aim is not to prove the mere fact of extrasensory perception. That she believes has already been sufficiently demonstrated by a long list of laboratory studies. Her interest lies rather in determining how far such processes occur in ordinary everyday life, and what are their general characteristics and their causes. To show, as Galileo did, that balls rolling down an inclined plane obey a definite law sounds rather like a trivial parlour-game carried out by a professor of mathematics; but, when the same law is found to govern the flight of cannon-balls and the orbits of natural and artificial satellites, the phenomena studied in the laboratory begin to have a practical value and widespread implications. Similarly the fact that a few exceptional persons are able to guess the numbers or the pictures on cards more frequently than a statistician would expect from sheer chance is in itself of little general interest; but if in their daily activities most people were at times liable to be influenced, in however slight or subtle a way, by the thoughts and emotions of others or by certain types of distant or future events, as a result of some agency hitherto unknown to science, that (we may readily agree) would be a matter of both theoretical and practical importance.

Readers familiar with the relevant technical journals will know that Mrs. Rhine has already subjected reported experiences of this kind to an elaborate technical analysis. The present volume is written in popular language and intentionally addressed to the man or woman in the street. Part of her object is to assure people who have such experiences that their occurrence is by no means exceptional and need not be regarded as alarming or uncanny: still less that a readiness to relate them to others is the symptom of an unbalanced, unscientific, or superstitious mind. After all, sceptical lawyers like Sir Edward Marshall Hall, philosophical agnostics like Gilbert Murray, Fellows of the Royal Society like Sir Alistair Hardy and Sir Arthur Eddington, and even a President of the Royal College of Physicians like Sir Risdon Bennet, have frankly confessed to experiences of this kind.

Both the American and the British investigators begin with the familiar threefold classification into cases of telepathy ('from other minds'), clairvoyance ('from mindless objects'), and precognition ('from the future'), with a further category for 'miscellaneous' and 'general' cases. It appears, says Mrs. Rhine, that "the main types of ESP expressed in nature agree with the divisions of ESP found in the laboratory ". The British data include fewer examples of telepathy than the American, but far more examples of precognition; and an application of the chi-squared test shows that the differences are fully significant.

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[1] A visitor just back from Leningrad has declared that the recent Russian determination to study parapsychology seriously is due to the fact that they are sure that American scientists can only be spending time and money on such projects because they fancy they may be on the track of a method of espionage which no enemy could frustrate or detect!

Both investigators cross-classify the data according to what Mrs. Rhine has termed their ' form '. The experiences may be ' realistic ' (i.e., they may reflect in photographic detail the events or facts to which they relate): this appeared to be the rarest type of all, occurring chiefly in precognitive dreams. They may be ' unrealistic ' (i.e., they may present the events or facts in distorted or imaginary form)—a slightly commoner type. They may be ' hallucinatory ' (i.e., they have all the sensory vividness of the most realistic dream, but by definition occur only in a waking state): this is the commonest type, and accounts for nearly half the classifiable cases. Finally, they may be ' intuitive ' (i.e., the event or fact comes to the subject's mind, not in sensory shape, but simply as an idea or thought): this is reported in only about 15 per cent of all the cases; it is less dramatic, and so (one is tempted to suggest) might easily get noticed less frequently. The instances of apparent telepathy, it would seem, were usually ' intuitive '; the instances of apparant clairvoyance usually hallucinatory.

In both the inquiries reports from women greatly predominated, the proportion being about 3 to 1 in the American and 4 to 1 in the British. In Mrs. Rhine's view the difference merely means that women are generally less shy or reluctant in reporting such experiences. Paranormal experiences may occur at almost any age, including early childhood. They seem somewhat rarer among children and among the elderly; the peaks apparently occur at 25–30 years among the men, and 50–55 years among the women. Actually the proportions at different ages do not differ greatly from the proportions of the several age-groups in the general population. Slight though they are, however, a chi-squared test shows that the deviations are statistically significant.

The commonest (but by no means the only) topics with which the experiences reported were concerned appeared to be those of threatened danger: death was the subject in nearly 30 per cent; non-fatal accident came next. Again, we may note, these are the most dramatic of the topics mentioned. In a small proportion of cases some kind of motivation seemed discernible on the part of the agent; but it was far more obvious in the case of the percipient. Where agents as well as percipients could be identified, it appeared that the percipients were usually female and the agents more frequently male. For the commoner types of family relation (naming agent and percipient second) the order of frequency was—in the American investigation, son-mother, two friends, brother-sister, mother-daughter, two sisters, daughter-mother, husband-wife, father-daughter; in the British, two friends, son-father, son-mother, husband-wife, uncle-niece, mother-daughter, brother-sister. The size of the samples, however, as Miss Green is careful to point out, are too small for the orders or their differences to be fully significant (in the statistical sense).

In some of the hallucinatory experiences that take the form of ostensible communications from the dead (or in other instances from the living) the person who so vividly communicates the warning could have no obvious reason for singling out this particular recipient: sometimes indeed (as in one or two cases that have come my way) the supposedly dead transmitter was actually alive; in others he very probably never existed.[1] Occasionally the personality of the communicator seems to be built up in the course of the experiences themselves, supplemented no doubt by frequent reflections on their nature. Nearly always, he, she, or it is a person or object of authority to whom the recipient had naturally turned for comfort, for protection, or for guidance either by the spoken word or by an exemplary way of life. Plainly, however, those readers who have 'immortal longings' must look elsewhere for their 'intimations'. Moreover, contrary to popular notions, hallucinatory appearances of the living are, in almost all the surveys, far commoner than hallucinatory appearances of the dead.

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[1] E.g., cases in which the ostensible communicator was a legendary patron saint or a primitive idol; with children the communicator is sometimes a pet or a doll cherished in early childhood. The influence of personal or cultural background is equally obvious in the case of religious mystics. The earlier Hebrew patriarchs hear the voice of Yahweh; the later prophets hear or see an angel, as the Virgin Mary is reported to have done; St. Paul and numerous Christian saints see the risen Jesus; others the Virgin herself; Socrates hears his daemon; Plotinus sees the Celestial Venus; Suso the Eternal Wisdom; Blake the strange personages of his prophetic books; Swedenborg the equally strange spirits belonging to his own ' celestial hierarchy '; St. Teresa (whose style, as Thouless has observed, suggests that she was a non-visualizer) states that her revelations always came in non-pictorial form; with Richard Rolle they came in the form of significant melodies.

Dr. Rhine accordingly infers that the percipient is more frequently the real originator of the experience than the agent. Many of the stories strongly suggest that states of 'heightened emotion' and of 'relaxed attention' were important predisposing factors. Miss Green points out that the circumstances described often imply a condition of temporary expectancy, not necessarily concerned with the subject of the experience: time after time the narratives explain that the percipient was 'just about' to go for a walk, enter a house, or receive a visitor. Most of the correspondents evidently assume that extrasensory phenomena genuinely occur, and many interpret them in crude spiritualistic terms. Reports from convinced sceptics are occasionally mentioned, but seem far more rare.

Manifestly a heterogeneous set of anecdotes gathered in this way can claim little evidential value. Miss Green and her collaborators endeavoured to allot an 'evidential score' to each of the narratives. Those analysed amounted to 300, selected out of about 1 500 as being sufficiently detailed for systematic study. Of these none reached an adequate standard as judged by the recognized criteria; but 17 (just under 5 per cent) obtained moderately high marks. A large proportion did not even profess to be veridical; and of those that seemed to correspond with some relevant event nearly one half appeared to be fortuitous coincidences. But it would be a gross mistake to criticize the investigators for depressing their standards or lowering their sights. As we have seen, their aim was, not to seek evidence for verificatory correlations between paranormal experiences and external events, but merely to make a preliminary survey of the general nature of such experiences, and to try out the kind of statistical analysis likely to be profitable in the more elaborately planned inquiries of the future.

However, there is, in my view, a still more serious problem. To describe one's inner experiences, particularly when they are in some way unusual, calls for accurate introspection; and that in turn demands an appropriate training and a suitable technical vocabulary. I believe therefore that we should examine these narratives rather as a clinical psychologist would approach the study of his patients' dreams. We must distinguish the 'latent content' from the 'manifest content', and remember that the latter is very apt to assume a concrete and symbolic guise which the narrator nearly always takes at its face value. Those who are interested in determining to what extent such experiences are veridical should, I suggest, base their comparisons not so much on the manifest or superficial content as on the latent. Moreover, when the dream is related after an interval of time, it usually undergoes what Freud used to call "sekondäre Bearbeitung". Quite unintentionally the narrator tends to revise or reorganize it, so as to give it a more picturesque, dramatic, or logical form, and to mould it more closely to his personal preconceptions or his theoretical views. Hence it is always difficult to know how far these recollected anecdotes can be accepted as trustworthy accounts of what the subject really experienced. Even that does not wholly rob the analyses of their interest for the psychologist. It does imply, however, that we must think of them as primarily a study of what various people of different cultural backgrounds are prone to report, and only at a second or third remove as a study of what people have actually seen, heard, thought, or felt.

Nevertheless, in spite of these difficulties, Mrs. Rhine maintains—rather in the spirit of William James—that, when full allowance has been made for these distorting factors, we can discern, underlying these various reports, the operation of 'lawful processes'. They suggest (she believes) that human capacities are far wider and the universe much richer than "sense-conditioned mortals in a mechanistic age" have hitherto been disposed to assume. Above all, they indicate, however vaguely, the possibility of a "genuinely scientific approach to this larger area". But, whether or not we as psychologists share these hopeful expectations, we must, I think, agree that both she and Miss Green, with their several collaborators, have shown that surveys and analyses of this kind are well worth undertaking. The study of dreams and day-dreams, of the wild delusions of the insane, and the curious illusions to which even the normal are subject, has already shed a flood of light on some of the most baffling problems that confront psychology; and similarly, even if these so-called paranormal experiences themselves turn out in the end to be perfectly natural and normal, we are bound to welcome any serious attempt to collect and classify the data they provide.

CYRIL BURT

Clinical Process. By E. KUNO BELLER. New York: Free Press of Glencoe, 1962. Pp. xx + 394. $10.

This volume, we are told, is concerned with "the study, treatment, and education of the young child and his parents". It "began with a research study, and grew into an attempt to synthesize theory and practice as well as clinical and quantitative methods". The author's aim was twofold: to provide first a "framework for organizing and recording clinical data", and secondly, "a method for the quantitative assessment" of such data with a view to furthering their use in research. The book is subdivided into five main parts dealing respectively with 'clinical records in practice and research', 'the initial phase of the clinical process', 'diagnosis', 'treatment', and finally 'clinical records as data for research'.

As a practical guide for the type of clinical worker envisaged, the volume, with certain limitations, would no doubt be excellent. The author evidently assumes that such a worker will not possess much scientific background or be capable of a thoroughgoing scientific attitude, but would be able to interview parents, teachers, and others provided he is furnished with a detailed mechanical scheme indicating the type of questions to be asked and the way the information is to be recorded. At certain points in the text a dozen tabulated 'guides for recording' are introduced and discussed at length. To judge by the verbatim quotations from some of the specimen reports, their command of simple, intelligible, and grammatical English is sadly defective. The author himself seems a little lacking in humour, and prone to mixing stereotyped metaphors. Of one mother it is said that "In marrying her goal was to have roots"; of a father all we are told is that "Mr. K. has made little progress as usual; his resistance has not lessened"; of another "he was not a joiner" (this describes not his occupation, but his "current family functioning"); and the child "R. has behaved more grown up". The interviewers evidently do their best to translate what they observe into the recognized jargon: one child, it is stated, "moved his fingers in an autistic manner"; in another case, "the treatment has been ego-supportive". Of the results we learn that both mother and child "permit each other to function more as an autonomous individual", and of another we hear that "she becomes disorganized when through with an activity." Unfortunately, the effect of these American textbooks has already been to introduce a mass of pseudo-scientific, would-be-euphemistic phraseology in the British clinical psychology, which has tended to make it a butt for much ridicule on the part of those who have to read or listen to such reports.

Dr. Beller's scheme for diagnostic classification seems to be based, not on any systematic theory of the nature or structure of mind or personality, such as might be verifies by factor analysis, but on a rough percentage analysis of the diagnoses actually made at various clinics. "To facilitate teaching and training of students," so the preface tells us, "the organization of clinical data is preceded by a discussion of basic concepts." But in point of fact it is by no means easy to discover what concepts are regarded as 'basic'. The book starts by explaining that "the concept of personality disorder is as yet far from a precise definition, but there has been constant progress towards this goal". However no formal definition of this or any other of the 'basic concepts' is attempted; and highly technical terms are frequently introduced with no explanation at all or with a vague discussion in semi-metaphorical terms. "The psychological examination," it is said, "consists of a battery of tests, selected to evaluate specific areas of functioning." Several tests are recommended, but what precisely they are intended to test is never clearly stated. True to the behaviouristic approach the idea that differences in general ability or special aptitude are largely determined by the individual's innate constitution is never even suggested. The Stanford-Binet is said to test 'intellectual functioning' (whatever that may mean); the I.Q. 'tells us whether the child functions more or less effectively than the typical child of his age'; and 'validity' is to be assessed by comparing 'aptitude test findings' with achievements.

It will be seen therefore that, as an aid to those engaged in scientific or statistical research, the book has little or no value. The author appears to believe that a 'quantitative evaluation' of records obtained by the methods he outlines would form a welcome contribution to psychological knowledge. But the method of evaluation he has in mind seems to consist simply in calculating percentages for the various items in his list. The need to use a control group or to test the statistical significance of the differences found is never suggested. There are so many would-be investigators who believe that this is all that statistical research entails that an emphatic protest seems desirable. Such percentages form an interesting part of the annual reports of clinics and they may occasionally suggest research problems. But it cannot be too strongly stated that, with rare exceptions, the data used in research must be data collected for that research, and not consist of material collected merely for the practical purpose of diagnosis and treatment. A research must be planned from start to finish with an explicit problem in mind and with the statistical methods of analysis clearly in view from the very outset.

C. F. HOLDEN

Darwinism and the Study of Society: A Centenary Symposium. Edited by MICHAEL BANTON. London: Tavistock Publications, 1961. Pp. xx+191. 21*s.*

The twelve essays included in this volume are based on papers read at a Conference organized by the Scottish Branch of the British Sociological Association to celebrate the hundredth anniversary of the publication of *The Origin of Species*. The semi-centenary of Galton's death, and the recent discussions at psychological conferences of Galton's applications of Darwinism to social problems, make the appearance of the book particularly opportune. Ever since Darwin's great work was published, so the jacket assures us, "there has been an influential school of thought maintaining that heredity, in its interaction with other factors, determines the major features of social life; but critics of this viewpoint maintain that such 'Social Darwinism' ignores the most distinctive characteristics of human nature and takes us back to the law of the jungle". That in a nutshell is the criticism put forward by most of the contributors to this symposium.

As might be expected, the contributions themselves vary considerably in intrinsic value and in their bearing on the problems of psychology. To the psychologist probably the most interesting papers are those on 'the human evolutionary system' by Professor Waddington, on 'social mind and animal brain' by Professor Farrington, on 'evolution and history' by Mr. Maynard Smith, and on 'Darwin's place in the history of thought' by Professor Basil Willey.

Dr. Bronowski has written a lively introduction. With much justice he criticizes the way in which modern sociologists—including some of the contributors to this symposium—tend to regard the term 'Darwinism' as "standing merely for a mechanism of social struggle"—sometimes a struggle between competing persons, classes, or nations, sometimes a struggle between competing ideas, ideals, ideologies, and technologies. An interpretation of this kind, as he rightly observes, is to "let the tail wag the title, and to make the explanation more important than the subject to be explained". After all, the notion of a progressive struggle in which only the fittest survive was not peculiar to Darwin. We find it in Swift, Defoe, and even as far back as Mandeville's *Fable of the Bees*. The special value of Darwin's work, so Bronowski tells us, lies in its emphasis on the essential parts played by individual variations and (what seemed still more shocking to his own generation) by blind chance. His discussion of variation gave rise to the theory of the 'gene-complex', as elaborated by twentieth century Mendelians; his emphasis on chance to the "application of statistical techniques, as developed by Galton and Karl Pearson". After this, we might expect to find in the papers that follow some account of the bearing of genetical theories on problems of social psychology (along the lines, for example, of Darlington's recent work) and of the illuminating results of statistical research (along the lines of Fisher's investigations into social class, social selection, and human fertility, and of his examination of 'sociological views' in his *Genetical Theory of Natural Selection*). Instead what we get are very largely abstract discussions of loose analogies between biological processes as interpreted by Darwin and historical or social processes as interpreted by the sociologists themselves.

Darwin, I imagine, would have retorted that his main contributions to the study of sociological problems were to be found in his demonstration that the mental processes of human beings—their abilities on the intellectual side, their instincts on the motivational side—were subject to the same laws of variation and inheritance as their physical characteristics. Yet, strange to say, these implications of Darwin's work are either wholly ignored or waved away as entirely out of date. Professor Ginsberg, for instance, whose ideas on the subject seem of late to have been appreciably altered by behaviouristic doctrines, maintains that "years of controversy have led to the view that the principal factors in social change are social rather than genetic". Dr. Werner Stark discusses in some detail 'Social Darwinism' as represented, for instance, by Otto Ammon's book on *Die Gesellschaftsordnung und ihre natürlichen Grundlagen* (1895). One corollary, he says, which Ammon "drew from his basic Darwinism was a definite anti-egalitarianism," supplemented by a number of plausible reasons why "a class society is better than a class-less one". Thus, "in a class society all children can receive the education best suited to their hereditary equipment". Such a statement of course flies in the face not only of behaviourism but of the democratic principles which nearly all the contributors wish to support. "We see here", concludes Dr. Stark, "looming up behind Ammon, not only Herr Himmler, but the gas chambers of Buchenwald and Auschwitz, which were meant, in the spirit of this philosophy, to help Nature do her wholesome work"—the work of Darwinian selection by eliminating the unfit.

Ammon, of course, like Darwin and Galton, wrote long before modern genetical theories and modern statistical techniques had been developed. It is therefore easy to find fault with his rough and ready assumptions. Contemporary authorities of greater eminence, who represent the same school of thought, such as Fisher and his co-workers, are dismissed in a line or two. Fisher is bracketed by Lancelot Hogben with another of Hitler's 'philosophers'—the late Alfred Rosenberg: and Fisher's detailed analysis of the evidence which indicates that "genetic selection has played a definite part in the decline of civilizations" is brushed aside with the curt remark that "it is reckless to assert that it has done so without a searching examination of other possibilities"—which seems about as logical as denying my assertion that I have got ten shillings in my right-hand trousers pocket because I have not yet made a searching examination of all my other pockets. In a later paper Dr. Banton mentions the fact that Professor Darlington has elaborated very similar views; but instead of making 'a searching examination' of Darlington's evidence, Dr. Banton prefers to devote three pages to refuting a speculative discussion written thirty years ago by Arthur Keith. This indeed is typical of the general procedure: you choose as 'an excellent example' of the 'Darwinian arguments' some particularly foolish or fanciful specimen published by some dead author, and you expect your readers to suppose that you have thus disposed of all the evidence there is; you talk of the importance of 'quantitative studies' and 'statistical research', but you offer none of your own.

Several of the contributors spend a good deal of time demonstrating that " since the dawn of civilization the human species has undergone little or no change as a result of natural selection "; and that " social evolution has taken the place of biological evolution ". But these propositions, I imagine, no social psychologist today would wish to deny. We may readily agree too that, with a few notable exceptions, there is comparatively little difference between the average innate mental capacity of different ' races ' or ' ethnic groups '. There is however some evidence that the *range* of variation may vary; that within the species as a whole, individuals, classes, and nations have been continually subjected to the processes of natural selection; and that such factors as differences in the birth-rate still exercise a potent influence on the rise and fall of social communities.

In a Conference organized by the Scottish branch one would have thought it difficult to ignore the work of Sir Godfrey Thomson and his collaborators on the effects of differential fertility. Only Dr. Bronowski, however, actually mentions the Scottish population surveys. Thomson and his school, he says, argue that, because the dullest parents generally have the largest families, the intelligence of the population as a whole must be falling. Dr. Bronowski then declares that the " two massive surveys of the school children of Scotland have now shown that the facts do not support [this conclusion]: on the contrary, the more recent survey discloses a significant rise ". And he deplores the way in which the surveyors decline to accept this conclusion, because (as he says) it presented an " unwelcome " conflict with their own preconceptions. This, however, is grossly unfair. Thomson was honestly ready to accept the facts, whatever they were. But one of the facts the investigators had to face was the way in which (so they discovered) both teachers and children had become increasingly familiar with the device of intelligence testing during the interval between the two surveys. In 1947 the group tests used were identical with those which had been adopted in 1932. Many teachers doubtless guessed that the new tests would at least be similar to the old, and probably analogous to those often used for entrance to secondary schools, such as the 11-plus examinations. Some teachers not unnaturally felt, as teachers often do, that they would not be doing justice by their own pupils if they did not give them preliminary practice. And, even apart from this, pupils had grown increasingly familiar with techniques of mental testing.

For some of the batches, however, the individual tests were changed; and, when the marks were reduced to terms of a comparable scale, it was found that the average mark for the boys had increased by no more than a fraction of an I.Q. point, while the average mark for the girls had actually decreased.[1] Dr. Bronowski fails to note that the interval between the two surveys was only 15 years—far too short for the differential birth-rate to have exercised any plainly demonstrable effect. Thomson therefore was fully justified in maintaining that the results provided no real evidence for any change in the average level of intelligence, either upwards or downwards: and that this was a question which could only be solved by later surveys. The main purpose of this second survey, as the preface points out, was to determine whether the alleged negative correlation between intelligence and size of family ' really exists '; and that certainly, as Thomson says, the inquiry fully confirmed. With the group test the correlation was $-0{\cdot}28$, with the individual test $-0{\cdot}32$. Incidentally, a vast array of illuminating information was collected on a number of social issues, which was later analysed and published in a much fuller report on the *Social Implications of the* 1947 *Scottish Mental Survey* (1953).

In his concluding paper on ' the autonomy of Post-Darwinian Sociology ' the editor briefly sums up the general outcome of the conference. Modern sociology, he tells us, operates with two frames of reference: one is the quantitative analysis of social characteristics, illustrated by social mobility, etc. ; the other, the ' approach from field investigation ' and the analysis of case material. The sorts of problems with which the sociologist is occupied are the determinants of occupational prestige, the effect of class differences on educational opportunity, and so on. Such questions, he contends, are " independent of biological problems; they must therefore be pursued separately ". But this seems a curious postulate to be laid down on *a priori* grounds. Whether or not biological factors such as heredity and variation have any influence on social mobility, class differences, educational opportunity or the like—and, if so, how great that influence is—these are surely questions of fact, to be settled by first-hand investigation, not by armchair arguments. We may readily agree that the early Darwinians in their enthusiasm frequently exaggerated the importance of biological factors. But here the pendulum of fashion has swung much too far in the opposite direction.

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[1] See for a fuller discussion of the points ' The Trend of Intelligence ', *Brit. J. Educ. Psychol.* XX, 1950, pp. 57f.

M. R. REED

ERRATA

In the article " Factor Analysis, Latent Structure Analysis, and Mental Typology ", by Miller, Eyman, and Dingman (Vol. XIV, 1961, pp. 29–34), the following errors should be noted. In Table III, the class II loading for delinquency should be ·02, not ·72. In Table IV, the designation of the positive response for Occupation should read ' low ' not ' medical '. This alters the second and third sentences of the paragraph just above Table III where it should now be stated that delinquency and high socioeconomic status give results which are consistent with the latent class analysis but not with the results published by Sabagh.

INDEX TO VOLUMES I TO XIV

An index to the first fourteen volumes of the *British Journal of Statistical Psychology*, classified according to authors and subjects, is now available. As proprietors of the journal the British Psychological Society wish to take this opportunity of expressing their gratitude to Mrs. Beatrice Warde to whose generosity and expert assistance the compilation and printing of the index is due.

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November 1962

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RELIABILITIES AND VALIDITIES OF SIMPLE AND EXTENDED WEIGHTED AND BUFFERED UNIFACTOR SCALES

By Raymond B. Cattell and John A. Radcliffe
University of Illinois and University of Sydney

Formulae are required for estimating the reliability and validity of scales constructed to measure a single factor, when irrelevant common and specific factors also exist among the data. Equations are here developed for this purpose, to cover scales with single members or many members, and with both weighted and unweighted components: the use of suppressor patterns is also taken into account. Relations to existing formulae, e.g., those of Spearman and Brown, are worked out, and an improved nomenclature is proposed.

I. Problem and Proposed Solution

The properties of scales designed for factor-pure measurement differ from those of ordinary homogeneous or unidimensional scales. The latter can be constructed without prior knowledge of personality and ability structure. Hence psychometrists interested in scale construction—Cronbach, Flanagan, Gulliksen, Kuder, Loevinger, Lord, Mosier, Richardson, and others—have built up a highly developed literature on this topic. However, it is necessary to distinguish this development from that which has become possible through researches by factor analysis in the fields of ability [10], personality, and motivation [2, 3]. Now that it is possible to design scales for uniquely rotated, psychologically meaningful, and experimentally practicable factor concepts, it becomes desirable to examine the reliability, validity, and variance structure of scales of this type. Some such study is urgently needed as a basis for primary ability measurement [10], for questionnaires and tests on personality [3, 6], and for such clinical factors as anxiety [5]. The present senior author has suggested [3] the term *item-metrics*, for the scaling theory that hinges on the use of items as atoms in a model having no reference to psychological structures *per se*, and the term *factor-metrics* for techniques relating to structural concepts which do not depend merely on the items and item arrangements given in the test material. Here then we deal with factor-metrics; and our intention is to relate the formulae and terminology required in this field to those already existing in item-metrics.

A new and more comprehensive terminology is quite as essential as new formulae. In an initial treatment of unifactor scales [4], dealing with the modifications required in the Spearman–Brown formula, a good deal of misunderstanding has arisen through the use of ‘ specific factor ’ both for a psychological specific and for a factor specific to a scale or member. This is but one

2 7 SEP 1962

instance of the need for a clearer nomenclature. In addition, we shall describe a model better adapted to present resources for constructing uni-factor scales than the existing models for homogeneous scales based on item-metric formulae. In particular, we shall develop formulae based on the following assumptions, which are altered from those of existing reliability and validity formulae:—

(1) Most factor batteries are not homogeneous. For example, the neuroticism factor scale in the Objective-analytic Personality Battery [3] contains one sub-test of sway-suggestibility, measured in centimeters, another of accuracy-speed ratio measured in errors of performance, and another of endurance measured in seconds. All these sub-tests have different specifics and a number of unwanted common factors. In batteries such as these therefore high homogeneity is not sought, and cannot be postulated for the associated formulae.

(2) Almost invariably, simple-structure factors show moderate but systematic mutual correlations. The correlations of real factors must, so far as possible, be retained in the factor scales. Orthogonality is therefore neither an assumption nor an ideal in the scales we now envisage.

(3) At the present stage of personality and ability research it is practically impossible to discover *well-loaded* sub-tests sharing the desired common factor only. Behaviour that is not trivial but gets out into personality space tends to be factorially composite. Valid items or sub-tests may share as many as a dozen contaminating factors. The assumption that an item can be conceived as made up of one common factor plus a specific survived simply as a result of the prevailing practice of using small batteries, in which broad group factors may be represented by only a single sub-test. By thus turning his back on these intruding, extraneous psychological factors or dismissing them as specifics, the theorist does not prevent his scale measurement from being warped toward other meanings. If our statement of present resources is correct, the proper procedure is to include sub-tests each of which will have the highest possible loading in the wanted common factor despite the various unwanted factors, both common and specific. Steps can be taken to reduce the effects of such factors by introducing ‘ suppressor patterns ’. Accepting this aim, the task of the present article will be

(*a*) to define a terminology for precise discussion of unifactor and homogeneous scales and their relations to the sub-tests;

(*b*) to develop formulae for the reliability and validity of such scales, whether orthogonal or oblique;

(*c*) to relate these formulae, where desirable, to similar item-metric formulae concerned with homogeneous, unidimensional scales (‘ vector-pure ’, but not ‘ factor-pure ’);

(*d*) to work out reliabilities and validities for extended scales;

(*e*) to develop formulae for effective ‘ suppressor patterns ’.

II. NECESSARY DEFINITIONS

An important difference between the conceptual treatment of item-metric and uni-factor scales arises from the fact that the latter are embedded in the general psychology of personality structure. In terms of the two models this means that items in homogeneous scaling can be handled as so many counters in a vacuum, having nothing but statistico-mathematical properties, whereas the factor-scale model may be compelled to modify itself with further discoveries in psychology; for here we deal with the behaviour of psychological, not merely mathematical, factors. This both imposes more restrictions and yields wider psychological implications. For example, we cannot assume that items consist simply of the required factor and a specific, while the implications of validity extend beyond the given factorial system—from R-technique (say) to corresponding P-technique source-traits [2, 3]. Incidentally, the item-metric framework has been responsible for much overvaluation of reliability (of the internal consistency type) relative to validity, and an unhealthy separation of psychometrics from psychological theory.

Our first terminological need arises from the fact that the terms general (or common) factor, specific factor, etc., must be considered in relation to what are known to be general and specific factors in the psychological world, whereas in item-metrics 'general' and 'specific' acquire their meaning from the universe of variables in the particular scale under discussion, and may therefore change from one study to another. Certain factor concepts must therefore be defined at the outset.

Let us begin by defining a unifactor scale as a measuring instrument designed to give the most reliable and valid measurement of an experimentally well replicated, unique rotation of a psychological common factor. 'Scale' can be synonymous with 'battery', and will consist of a measuring instrument divisible into sub-tests or items, which may or may not be verbal items.[1] In discussing the relations of longer to shorter tests we shall use the word *member* for any component (sub-test, or item, or fractional scale) of the full scale. In general a member will be considered divisible into still smaller members, and a scale extensible into longer scales. The limit to divisibility of a member will be reached in an ultimate 'atom' which we shall call a *stimulus-response score unit*, or SRS, e.g., a verbal item, a reaction time, or a change of speed. ('Score' is used because the limit is set as much by what can be scored as a unit as by any internal unity in the response *per se*.) Distinguishing this SRS atomic limit of divisibility from a 'member' is important psychologically because the reliability of any SRS unit is well below that of a sub-test; it is important statistically because SRS units of unequal difficulty can never have equal standard deviations.

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[1] Theorists in mental measurement have too frequently limited their thinking, and the potency of their generalizations, to scales made up of verbal items. Hence we must remind the educational psychologist that members may often be whole sub-tests, of different apparent nature, and that such sub-tests, even when of appreciable length, may not be divisible into 'items'.

difference between two scores due to error), or the stability coefficient (which includes function fluctuation of the trait within the person), or the consistency and homogeneity coefficients (which are really statements about internal test structure rather than reliability).[1] However, it seems best to base our treatment here on the most fundamental of all reliability coefficients, namely, the dependability and equivalence coefficients.

The former is the correlation of the test with itself when re-administered after a time interval too short for function fluctuation to have occurred, and after previous administrations have made practice effects negligible. It falls short of unity by the experimental error of measurement inherent in administering that kind of test. The equivalence coefficient, on the other hand, has the disadvantage of being somewhat unreal, in that it assumes the psychologist can always construct a second test with exactly the same unifactor[2] loading as the first. Moreover, as at present defined, it proves to be highly ambiguous, since it does not clearly indicate whether the nonunifactor variance in the ' equivalent form ' is provided by unwanted common factors *plus* a different specific, or by a different specific only. The latter is more commonly assumed: the former is more likely to be achieved. Accordingly, to distinguish the two, we shall speak of *alpha* and *beta equivalence coefficients*. The beta coefficient represents the correlation between forms constructed so as to have the unifactor as the only shared (and *equally* shared) factor, while the alpha coefficient will be the correlation between forms having wanted and unwanted common factors as well as specifics in the systematic variance of both.

How nearly miraculous the construction of beta-equivalent forms would be can only be decided by further experience in test construction. The most likely place should be among different sub-tests, such as occur in the Objective Analytic Personality Test factors, e.g., fluctuation of attitudes and susceptibility to annoyance, in U.1. 24 (Anxiety), or fluency on self and errors on the C.M.S. test in U.1. 32 (Extraversion). On the other hand, alpha-equivalent forms should not require anything more than sheer good workmanship for their realization, once a set of carefully factorized potential members and S.R.S. units has been accummulated.

This brief discussion may suffice to remind us that the definition of some test coefficients depends not only on stipulating the correlational operations, but also on the conditions of test construction and administration. As elsewhere [3] we shall symbolize the dependability and equivalence reliability coefficients defined above as r_d and r_e. Subsequent treatment will refer exclusively to the alpha form of the latter.

[1] As Cronbach points out, they fall short of unity both because of error variance and because of specifics and unwanted common factors [8]. In any case we cannot let the general argument hinge on the special existence of split-half or multiple division internal consistency coefficients, for the member measured might be an SRS or otherwise unfissionable item, not capable of yielding such coefficients.

[2] The term ' unifactor ' will henceforth be used as a brief synonym for the ' *wanted* common factor in the given scale '.

IV. THE RELATIONS OF VALIDITY AND RELIABILITY COEFFICIENT TO SCALE COMPOSITIONS

We shall now derive reliability and validity coefficients for a single member from the expression for its factorial composition. Later we shall generalize the formulae to the scales created by extending a test to n members. We shall make the minimum assumptions; and, instead of postulating a restriction to orthogonal factors, we shall include them as a special case.

An extended scale will be a scale composed of n members $(j, k, \dots, n)$. For the scale composed of single member we shall assume the most general model, namely, that scale variance results, from a *wanted* factor (f), a number of *unwanted* factors $(u, \dots, y)$, factors of *error* (e), and a factor *specific* to the member (s).[1] We desire to measure validity against the *wanted* factor, which will be a factor common to *all* members.[2] The other *unwanted* factors need not be common to all members, and may be correlated with each other and with the wanted factor.

Let f_j, u_j, v_j, $\dots$, y_j, s_j, and e_j denote the *factor loadings* (pattern values) of test member j with the common, specific, and error factors respectively, and r_{fj}, r_{vj}, $\dots$, r_{yj}, r_{sj}, and r_{ej} the corresponding *factor correlations* (structure values). Then the standard score of an individual $(1, 2, \dots, N)$ for member j will be

$$z_{Ij}=f_jz_{If}+u_jz_{Iu}+\dots+y_jz_{Iy}+s_jz_{Is}+e_jz_{Ie}. \tag{1}$$

The variance of member j will be

$$\sigma^2_j=\sigma^2_j(f^2_j+u^2_j+\dots+y^2_j+s^2_j+e^2_j+2r_{fu}f_ju_j+\dots+2r_{fy}f_jy_j + 2r_{uy}u_jy_j+\dots). \tag{2}$$

The intercorrelation between two members, j and k, will be

$$r_{jk}=f_jf_k+u_ju_k+\dots+y_jy_k+r_{fu}(f_ju_k+f_ku_j)+\dots + r_{fy}(f_jy_k+f_ky_j)+r_{uy}(u_jy_k+u_ky_j)+\dots. \tag{3}$$

From the formula for the variance of a sum, the variance of a scale of n members will be

$$\sigma^2_n=\sum_j^n\sigma^2_j+2\sum_j^n\sigma_j\sigma_kr_{jk},\dots (j<k) \tag{4}$$

where the components of σ^2_j and r_{jk} will be as shown in (2) and (3). When the factors are *orthogonal*, components involving correlations will vanish.

It will simplify subsequent formulae to assume that all members have *equal variances*. Provided the score units are not dichotomous stimulus-responses this is a reasonable assumption. With continuous scores the use of product-moment correlations (or their equivalents) in the factor analysis will have reduced all members to unit variance. For the member scores to be additive and so produce a factor scale score, each must be expressed in equivalent units. But with members which consist of indivisible, dichotomous, stimulus-response score units, to assume equal variance would assume equal difficulty[1], so that it would then generally be necessary to replace the variance terms where they are omitted in subsequent formulae. Assuming equal variance (4) becomes

---

[1] The term *specific* is intended to refer to a specific within the personality sphere, though operationally it happens also to be a *member* specific. We assume only one specific and one error factor in each member, though there could be more without upsetting our general formulae.

[2] Except a specially introduced *suppressor* member: see Section VII.

$$\sigma^2{}_n = \sigma^2{}_j \, (n + 2 \sum_{j<k}^{n} r_{jk}). \tag{5}$$

We have already decided (see Section III) that reliability refers to the *dependability coefficient* and α- *and β-equivalence coefficient*, and validity to *concept validity*. For the correlations in (4) and (5) above and in the extension formulae below average values should be used, and these can most conveniently be reproduced values.[2]

For the *dependability coefficient*, we now have

$$\begin{aligned} r_{dj} &= f^2{}_j + u^2{}_j + \ldots + y^2{}_j + s^2{}_j + 2r_{fu} f_j u_j + \ldots + 2r_{fy} f_j y_j + 2r_{uy} u_j y_j + \ldots \\ &= h^2{}_j + s^2{}_j, \end{aligned} \tag{6}$$

where h_j = communality. For the α-equivalence coefficient (the correlation between parallel forms in which the systematic variance includes both kinds of common factor variance) we have

$$\begin{aligned} r_{ej} &= f^2{}_j + u^2{}_j + \ldots + y^2{}_j + 2r_{fu} f_j u_j + \ldots + 2r_{fy} f_j y_j + 2r_{uy} u_j y_j + \ldots \\ &= h^2{}_j. \end{aligned} \tag{7}$$

Validity will mean the correlation of obtained scores with factor scores[3], i.e.,

$$r_{fj} = f_j + r_{fu} u_j + \ldots + r_{fy} y_j. \tag{8}$$

V. RELIABILITY COEFFICIENTS FOR EXTENDED SCALES

Extending the scale to include n members, we now assume equal member variances and equal variances for the two n-member scales. From (6) and (7) we then obtain statements for the common variance which lead to the following formulae coefficients.

For the *Dependability Coefficient* we have

$$r_{d_n} = \frac{\sum_{j=1}^{n} r_{dj} + 2 \sum_{j<k}^{n} r_{jk}}{n + 2 \sum_{j<k}^{n} r_{jk}}. \tag{9}$$

---

[1] It may be argued that only in these conditions can point-correlations be used, since only then are member intercorrelations expressed in comparable terms.

[2] Whenever member correlations are used along with factor loadings, they should be consistent with them, and the loadings are best taken as the estimates of the *population* loadings rather than *sample* values. We are here dealing with psychologically meaningful, unique, simple-structure-rotated factors; and with the present uncertainties as to the number and exact rotation of factors dependence can only be placed on the *mean* of replicated factorings. To R (the z-mean of actual correlation matrices) or R' (the reproduced correlation matrix from the mean factor matrix) the latter is preferable because the correlations will include 'residuals'.

[3] If weighted scoring is used, the validity becomes the correlation between the factor scores and the estimated factor scores, or the multiple correlation between the weighted composite and the factor, as in Section VI below.

For the *Extended α-Equivalence Coefficient*, we have

$$r_{en} = \frac{\sum\limits_{j=1}^{n} r_{ej} + 2\sum\limits_{j<k}^{n} r_{jk}}{n + 2\sum\limits_{j<k}^{n} r_{jk}}. \tag{10}$$

To bring out our assumptions and their implications, it will be instructive to compare formulae (9) and (10) with the Spearman–Brown formula [8]. This Spearman–Brown formula estimates the correlation of a test composed of (say) items j, k, l, with a parallel test composed of items p, q, r, using data obtained from the test of items j, k, l, only, and assuming

(*a*) $\sigma^2_j = \sigma^2_k = \ldots = \sigma^2_p = \sigma^2_q = \ldots$, and

(*b*) $r_{jk} = r_{kl} = \ldots r_{jp} = \ldots = r_{jr} = r_{kl} = \ldots = r_{kp} = \ldots = r_{kr} = \ldots$.

As Cronbach [8] points out, less restrictive assumptions, which allow the development of similar formulae, may be substituted, viz.

(*c*) $\sigma^2_{j+k+l+\ldots} = \sigma^2_{p+q+r+\ldots}$; and

(*d*) $r_{jk}\sigma_j\sigma_k = r_{jl}\sigma_j\sigma_l = r_{jp}\sigma_j\sigma_p = \ldots = r_{jr}\sigma_j\sigma_r = \ldots$.

In our own formulae the only assumption made is that of equal variances of members. We have avoided the assumption that the members will not be contaminated by unwanted common factors; nor have we assumed equivalence of members in other ways.

In comparing our formula with the Spearman-Brown formula, or such formulae as Cronbach's alpha and Flanagan's coefficients of extension, the definition of the reliability coefficient involved must be kept in mind. Our use of the dependability and α-equivalence reliabilities is adopted on the grounds of their readier availability and their wider utility. Their relation to each other and to extension values can most easily be understood from Tables II and III.

TABLE II. FACTOR COMPOSITION OF PROPOSED RELIABILITY COEFFICIENTS

Equivalence Type	Test Form	Factor Type Wanted Common	Unwanted Common	Specific	Error
β	I	**f**	**u, ... , y**	**s**	**e**
	II	**f**	**U, ... , Y**	**S**	**E**
		β-coefficient			
α	I	**f**	**u, ... , y**	**s**	**e**
	II	**f**	**u, ... , y**	**S**	**E**
		(Validity coefficient) (α coefficient) (Dependability coefficient)			

Note. Lower and upper case letters denote different factors

TABLE III—SIMILARITY OF MEMBERS IMPLIED IN CORRELATION COEFFICIENTS BETWEEN MEMBER AND EXTENDED SCALES

	General Formulae					Spearman-Brown Formulae		
Member Scales	$\mathbf{j}_1$	←Dependability→	$\mathbf{j}_1$	← α or β equivalence →	$\mathbf{j}_2$	$\mathbf{j}_1$	← Reliability (α or β equivalence) →	$\mathbf{j}_2$
Extended Scales	$\mathbf{k}_1$		$\mathbf{k}_1$	← α equivalence →	$\mathbf{k}_2$	$\mathbf{j}_3$	← β equivalence →	$\mathbf{j}_4$
	$\mathbf{l}_1$		$\mathbf{l}_1$		$\mathbf{l}_2$	$\mathbf{j}_5$		$\mathbf{j}_6$
	.		.		.	.		.
	.		.		.	.		.
	.		.		.	.		.
	$\mathbf{n}_1$		$\mathbf{n}_1$		$\mathbf{n}_2$	$\mathbf{j}_7$		$\mathbf{j}_8$

Note.—Identical letters denote identical common factors, with subscripts indicating different specifics; different letters imply similarity in the wanted common only.

It will be seen that they are quite different from those so-called coefficients which, strictly speaking, are merely measures of structure consistency or homogeneity ([3] p. 343).

The Spearman–Brown formula makes no explicit reference to the factorial structure of the items. Its relation to our reliability coefficients can be seen from Table III Its ambiguity arises from the fact that the homogeneity which makes the items correlate equally with one another can arise either from a single wanted common factor or from a combination of common factors. The elements are usually regarded as differing only in specifics (though, as Table II shows, these are partly psychological specifics and partly unlike common factors), and the 'reliability' which the formula measures is therefore, for most practical purposes, a β-equivalence coefficient. The first two members of α-equivalent scales, between which our correlation is calculated, have the same unwanted common factor and differing specifics. But when the α-equivalent scale is extended each new member may differ in its unwanted common factor, and it will invariably differ when the suppressor design is used.

The differences between the assumptions made here and those of other formulae may therefore be summarized as follows:—(1) r_{dj} is not r_{ej}; (2) neither r_{dj} nor r_{ej} equals r_{jk}; (3) neither the member intercorrelations nor the covariances within or between forms are assumed to be equal. The only assumptions are of equal member variances and equal total variances for the parallel forms.

If universally equal correlations are assumed (i.e., if $r_{ej}=r_{jk}$) formula (10) becomes the Spearman–Brown formula. If equal covariances are assumed, (10) reduces to the formula of Flanagan and others [8]. If the mean covariance between parallel members is assumed equal to the mean covariance between unpaired members, then (10) reduces to Cronbach's alpha [8]. Formula

(10) is thus a generalized formula, and could be made still more general by abandoning the requirement of equal member variances.[1] In this section we have considered reliability as unweighted. But if desired, the equations for weighted validity given in the following section can be applied to calculate a maximized member-weighted reliability.

VI. ACTUAL AND TRUE VALIDITY COEFFICIENTS FOR SIMPLE AND EXTENDED SCALES

The nature of validity for an *n*-extended scale remains the same as for a member, namely, concept validity. It is the correlation with the psychological concept defined by the unifactor. On the original assumptions—those which resulted in equation (8)—the correlation of the *n*-extended scale with the wanted factor, *f*, could be as follows. For Extended Validity we obtain two formulae

$$r_{fn}=\frac{\sum\limits_{j=1}^{n} fj+r_{fu}\sum\limits_{j=1}^{n} uj+\;\dots\;+r_{fy}\sum\limits_{j=1}^{n} yj}{\sqrt{(n+2\sum\limits_{j<k}^{n} r_{jk})}}\,; \tag{11}$$

and

$$r_{fn}=\frac{\sum\limits_{j=1}^{n} r_{fj}}{\sqrt{(n+2\sum\limits_{j<k}^{n} r_{jk})}}\,. \tag{12}$$

The latter is the more convenient to use in practice. When the factors are orthogonal, $\sum\limits_{=1}^{n} r_{fj}=\sum\limits_{j=1}^{n} fi$.

These equations represent *actual* validity. *True* validity may be conceived either (i) as that which exists between 'true' (i.e., perfectly reliable) scale scores and the factor, or (ii) as that which would exist with a test of infinite length. If correction for attenuation is conceived in the first way, the usual formula may be applied; if in the second way, the correction for attenuation cannot be applied with scale dependability or equivalence, because the derivation of the formula rests on the same assumptions as the Spearman–Brown procedure. It could be applied to *member equivalences*, and the corrected values could be included in (12) to obtain an estimate of *scale* validity, provided that the *members* had perfect equivalence reliability. In practice this is the most feasible way of estimating the extent to which scale validity could be improved by increasing member reliabilities.

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[1] Those who use these formulae should be careful to make the estimates of reliability appropriate, namely, dependability in (9) and equivalence in (10). Equivalence values in (9) would underestimate extended reliability, and substitution of dependability for equivalence in (10) would overestimate extended equivalence. Prior factor analyses are necessary to discover equivalent members before equivalence coefficients can be calculated. Estimates, however, can be based on assumptions (*c*) and (*d*), e.g., those of Guttman or Rulon ([8] p. 301), or (*e*) as in Cronbach's alpha.

So far we have assumed that the n-scale score is simply that which is obtained by giving equal weight to every member. Let us now consider the effect of weighted scoring. How are we to determine what particular weights which will maximize the multiple correlation with the wanted factor? The solution for the regression weights must here be the same as for estimating the factor from any collection of member scores. The only problems therefore are (*a*) to make the solution general, i.e., applicable to oblique as well as to orthogonal factors and (*b*) to avoid the practical difficulty of having to compute the inverse of a large matrix of correlations between members. Simplified methods have been suggested by Harman [14], Dwyer [9], Guttman [12, 13], Creager [7] and Thomson [17]. These all seek to avoid computing the inverse of the n-member correlation matrix; Harman's appears the easiest to apply. Basing their comparisons on weights obtained by Thomson's formula, Baggaley and Cattell [1] have shown that the use of the reference vector structure matrix for the matrix of regression weights yields factor scores which are a linear function of those derived from the actual regression weights.

Let $\hat{f}$ denote the estimated unifactor score from the n-member scale, and w_{fj} $(j, k, \ldots, n)$ the regression weight of member j, then:—

$$r^2_{ff} = R^2_{fjk\ldots n} = \sum_{j=1}^{n} w_{fj} r_{fj} = \sum w^2_{fj} + 2\sum_{j<k}^{n} w_{fj} w_{fk} r_{jk}. \tag{13}$$

The reliability of this weighted composite will be

$$r_{ff} = \frac{\sum_{j=1}^{n} w_{fj} r_{jj} + 2\sum_{j<k}^{n} w_{fj} w_{fk} r_{jk}}{\sum_{j=1}^{n} w^2_{fj} + 2\sum_{j<k}^{n} w_{fj} w_{fk} r_{jk}}, \tag{14}$$

which is equivalent to the formula derived by Mosier [16]. In the denominator (r_{jj}) may refer to either r_{dj} or r_{ej}, whichever has already been obtained. Except in the final stages of factor scale construction, the gain over unit weighing does not justify the trouble involved in calculating regression weights. An estimate of whether weighted scoring will appreciably increase validity may be obtained from a formula developed by Guttman [12, 13] for the orthogonal case and expanded by Creager [7] to the oblique, namely,

$$R^2_{f\cdot jk\ldots n} = \frac{\sum B_{fd} C'_{fl}}{1 + \sum B_{fj} C_{fl}}, \tag{15}$$

where $B = V^{-1}_{sp} V_{fs}$, $C' = V'_{fp} V^{-1}_{sp}$, V^{-1}_{sp} = inverse of specific factor pattern matrix, V_{fs} = common factor structure matrix, and V'_{fp} = transpose of common factor pattern matrix. In using this formula we encounter a new question. Is it more important to maximize the correlation of the scale with its unifactor or to maximize it only to a degree consistent with maintaining low correlations with other factors? This is a problem which does not arise in calculations with item-metric or homogeneous scales. As we shall see in the next section, the proper principle in constructing scales for meaningful factors is to keep scale intercorrelations equal to the population values for the intercorrelations of the true source traits with the given case, not to reduce them to zero. The correlation of an unwanted factor in a scale with the required unifactor is

$$r_{u\hat{f}}=\frac{\sum w_{fj}r_{uj}}{\sqrt{(\sum w^2_{fj}+2\sum w_{fj}w_{fk}r_{jk})}} \quad . \tag{16}$$

Although with weighted scoring $r_{f\hat{f}}$ may not significantly exceed r_{fn} (with unit scoring) r_{uf} (with weighted scoring) may be considerably less than r_{un} (with unit scoring). In this case the increased purity of the factor measure may more than compensate for the failure of the correlation with the wanted factor to increase with weighted scoring.

VII. The Effect of Suppressor Variables

If we are right in surmising that behaviour which has a large personality factor variance will generally be factorially complex, then the unwanted common factor variance will, as a rule, be far from negligible. Hence in constructing scales for the sixteen personality battery the usual practice has been to choose subtests which will reduce the contribution of unwanted factors by suppression. This method of eliminating variance by internal cancellation may be termed 'buffering'. Such expressions as a 'neutral', 'compensated', or 'counterpoised' scale might also be suggested. For the freeing of a scale from bias due to positive and negative loadings in unwanted factors is a near analogue to a 'buffer solution' in chemistry. 'Buffering' a scale is thus distinguished from 'purifying' it; the latter would involve throwing out contaminating items or intensifying the scale by replacing such items by others of a higher wanted factor loading. This technique has never been examined systematically in terms of its effects on reliability and validity; and we shall therefore attempt to do so here.

Guilford and Michael [11] have already examined suppressor actions. But their method differs from ours in the following ways. (*a*) It was restricted to abilities only and not extended to the realm of personality; (*b*) the factor scales were limited to orthogonal factors; (*c*) consequently the ideal adopted was to produce zero correlations among scales rather than (as here) the exact values existing among the pure oblique factors; (*d*) instead of aiming at complete suppressor action among existing variables, special variables, having negligible correlation with the required factor, were accepted. Our own aims are sufficiently indicated by saying that we wish to avoid the restrictions stated in (*a*), (*b*), and (*c*), and, in regard to (*d*), to achieve mutual suppressor action entirely through wanted items instead of by diluting the scale through introducing members for their suppressor action only. Experience suggests that variables can always be found in a large factored pool having the necessary complex factor constitution.

In combining suppressor profiles our first task is to reduce the algebraic sum of loadings on unwanted factors to zero. As shown below, this will reduce any correlation between the unifactor and the unwanted factor to a minimum. These minimal values should coincide with the correlations existing among the pure factors when the best simple structure has been obtained. 'Greatest validity' has two meanings: (*a*) Direct Validity, i.e., high correlation with the pure factor, and (*b*) Circumstantial Validity, i.e., behaviour in relation to other factors which shall be as similar as possible to that of the pure factor.

Determining the correlation of the factor scale with unwanted or ulterior factors can be considered first for the scale of n members, and secondly for a weighted scale, oriented as in (13) above to giving the maximum multiple correlation with the pure factor. The former is as follows:—

$$r_{un}=\frac{\sum u_j+r_{fu}\sum f_j+\ \ldots\ r_{yu}\sum y_j}{\sqrt{(n+2\sum r_{jk})}} \tag{17}$$

Since $\sum f_j>0$, the numerator of (17) will equal zero only when $\sum u_j=\ \ldots=\sum y_j =r_{fu}=\ \ldots\ r_{yu}=0$, or under the unlikely condition when the algebraic signs of the sums and factor correlations are so arranged to produce zero, so that complete orthogonal suppression would normally require perfect suppressor balance and zero factor intercorrelations. If perfect suppressor balance exists, but factors are oblique, r_{un} will approach r_{fu},. Correlations among the scales with n members will approach the true values as suppression and length are increased.[1]

The calculation of the correlation of the weighted factor estimate, $\hat{f}$, with the unwanted (which indicates the degree of success in buffering) will be based on the regression weights discussed in (13), and is given by (16) above. Setting $u_j, \ldots, y_j$ equal to zero in (16) we obtain a condition of nearly maximum suppression; but this will be only an approximation because the inclusion of suppressors will change r_{jk}, increasing it when the suppressors have loadings in the wanted factor, and decreasing it when the suppressors have loadings only on the unwanted factors. Suppressors may reduce the correlation with the wanted factor; and decisions as to their use require that the reduction in the unwanted variance be compared with the reduction in the wanted variance.

The practical effectiveness of suppressor patterns in the items of a multidimensional scale is considerable. This is shown by the fact that the scale scores, in the buffered 16 P.F., correlate very similarly to the pure factor scores.

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[1] Many scales to which these formulae will be applied will be scales associated with others in a whole battery, as in the Guilford-Zimmerman, etc. (4, [3], [6]). The above formulae may then be put into a comprehensive matrix form. However, good battery construction has usually avoided the situation in which all item scores contribute to all scale scores for the following reasons: (1) there are usually more items than can be simultaneously factored; (2) it is easy for common error to produce spurious correlations among items; (3) except with electronic computors scoring becomes quite impracticable if all items have to be taken into account for every scale; (4) most loadings would be almost negligible anyway, except on the wanted factor and one or two others. Consequently Cattell and Baggaley's postmultiplying matrix [1, eqn. 19] would normally have a predominance of zero entries.

This can be seen from the remarkable way the second order factors obtained by Karson [15] working on actual scale scores coincide with those of Cattell [3] working on simple-structure-rotated pure factors.

VIII. Summary and Conclusions

This paper is concerned with scales devised to measure a single replicated factor, such as is obtained by experimental rotation to secure simple structure and therefore factors that are psychologically meaningful. Our primary concern has been to develop suitable formulae for measuring reliability and validity, both with and without weighting. The formulae relate first to scales consisting of a single member and have then been extended to cover the commoner case of scales with n members. They are also amplified to allow for the use of items designed to secure mutual suppression of unwanted factors.

We have further compared the formulae thus constructed with those already developed for cases in which scale homogeneity rather than factor structure supplies the standard of reference. Our own formulae differ in demanding fewer assumptions. They coincide better with the real conditions of test construction; and the derivations are based on information more readily available in the form of coefficients experimentally obtained. However, with the aid of additional assumptions the formulae can readily be transformed to formulae of a more familiar type, such as the Spearman–Brown.

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THE INFLUENCE OF MOTIVATION ON THE RESULTS OF INTELLIGENCE TESTS

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In recent discussions about the borderlines to be adopted in selecting children for entrance to grammar schools or choosing students for entrance to universities it has often been maintained that the results of re-tests demonstrate that the standards commonly proposed are far too high. The figures here reported suggest that the discrepancies are due largely to the fact that the higher borderlines are based on test-results obtained during actual examinations, while the lower borderlines are obtained from investigations in which the individuals tested had no special motive for putting forth their maximum effort. And a detailed analysis of the data indicates that the reliability, validity, and level of performance are all appreciably reduced by the absence of any strong incentive when the tests are taken.

I. The Problem

In quoting and comparing results furnished by what are commonly called 'intelligence' tests psychologists and educationists are too apt to ignore the conditions under which the various results were obtained. Several investigators, for example, have applied group tests of intelligence to University students, and have noted that average scores even of Honours students often fall well below the borderlines that are commonly adopted when examining applicants for entrance [cf. 7, 8, 10, 11]. It is therefore inferred that the borderlines proposed are far too high, and that candidates with much lower marks could safely be accepted and expected to do well in their final examinations.

In an enquiry carried out by Miss Mawer and Burt [4] it was calculated that of the 709,580 persons aged eighteen in the years just preceding the last war 10,785 or just over 1½ per cent entered one of the English or Welsh universities.[1] Had these entrants been accurately picked out as the brightest 1½ per cent of their age group, then the borderline would have been equivalent to a deviation of approximately +2 S.D., i.e. (with an I.Q. scale having an S.D. of 15 points) about 130 I.Q. At that date, however, nearly half of the brightest pupils (particularly the women) did not go to a University; and first-hand investigations indicated that the actual borderline was nearer 125 I.Q., which was in fact the borderline frequently proposed in the period between the two wars by those who used intelligence tests as part of the entrance examination. Nevertheless, many writers—Leybourne-White, for instance—contended, on the basis of their own investigations of undergraduate or postgraduate students, that this criterion did not correspond with facts. And quite recently Dr. Furneaux has reminded us " that the late Sir Godfrey Thomson presented data to a conference of the Home Universities which showed that a quarter of all graduates had an intelligence quotient of less than 115 " (*The Times Educational Supplement*, 26 January 1962). From this some writers have inferred that in point of fact as many as 16 per cent of any given age-group should be capable of passing the University examinations required for graduation. Somewhat similar arguments have been used in discussions about the borderlines adopted both in the earlier examinations for junior county scholarships and in the more recent 11 plus examinations introduced for selecting pupils for grammar schools.

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[1] In this inquiry we were much indebted for figures and advice given to us by Sir Allen Mawer, at that time Provost of University College. The borderline cited in that earlier article (135 I.Q.) was based on a set of intelligence tests having an S.D. of 16 I.Q. points. In recent researches the tests used by most investigators have been slightly less discriminative, and the more usual practice nowadays is to translate results into terms of the conventional scale described in the test.

Now, since intelligence-tests, particularly when applied once only, have a reliability that is far from perfect, we should ordinarily expect the results of a second testing to exhibit a regression towards the general mean. When children with I.Q.'s under 70 or less were certified and transferred to special schools for the mentally deficient, it was quite usual for a second test to discover that a small proportion apparently had I.Q.'s well above the official borderline. If the borderline for degree students is set at +2·00 S.D., the mean of those picked out would be +2·37 S.D., with the test employed; if the tests had a reliability of 0·80, then the mean of the same group with the second test would drop to +2·02 S.D., and about half would fall below the original borderline. However, neither in testing the defective nor in testing the bright, is it possible to attribute all the discrepancy to mere regression in the test-measurements; and, when the sources of the discrepancies are more closely scrutinized, it frequently appears that the higher borderlines refer to scores that were obtained when the candidate's fate depended upon his own performance, whereas the lower figures were commonly obtained in the course of *ad hoc* researches carried out with volunteers, or at least with subjects who believed they had little or nothing to gain or lose from the way they exerted themselves during the tests.

Accordingly, to secure more precise information on this point, we have re-examined a variety of data procured from time to time both from students and from school children with a view to ascertaining how far (i) the reliability and validity of the tests and (ii) the means and the variances of the scores are affected by the conditions of motivation under which the different tests were applied.

II. Data from School Children

During the earlier years of the present century most of the intelligence-testing carried out on school children was undertaken in the course of personal interviews in which each child was examined *individually*. The object was to assess the intelligence either of the borderline pupils referred to the psychologist or school doctor for certification as mentally defective or of borderline candidates for a junior county scholarship whose performances in the written examination had, for one reason or another, failed to do them full justice. It soon became clear, however, that with the current practice a large number of cases towards either end of the scale were still very frequently missed. Accordingly, one of us [1, 2, 3] ventured to urge the need for a standardized screening by means of *group tests*. For the selection of scholarship candidates, it was suggested, written tests involving ' higher mental processes ' particularly those calling for the apprehension and use of logical relations—' opposites ', ' analogies ', ' completions ', ' reasoning tests ' and the like—could be used either as an adjunct to the traditional type of scholastic examination or as part of the ' preliminary examination '. In the earliest form of the tests the candidate was required to invent his own answers, and non-verbal as well as verbal problems were included [3]. Test problems of this sort, however, need fairly expert marking. Hence, when group tests were applied on a more extensive scale, the non-verbal tests were usually discarded, and alternative answers were printed in the booklets, the child being asked merely to underline whichever answer he thought the best.

The results obtained from children with group tests of both types are shown in Table I. The ' first series ' was obtained with verbal and non-verbal group tests of the kind described in *Mental and Scholastic Tests* with what are sometimes described as ' open answers '. The ' second series ' was obtained with the Northumberland Group Tests of Intelligence. With these the ' multiple choice ' method was used, and all the tests were verbal. The ' trials ' were carried out chiefly to standardize the tests before they were actually applied in the examination for junior county scholarships. The ' third series ' relates to

TABLE I. RESULTS FROM CHILDREN

	Number	Average	S.D.	Reliability		Validity
1st Series (Open Answers)				(*a*)	(*b*)	
Trial	234	99·3	12·8	·73	·69	·61
Examination	181	102·8	13·1	·85	·80	·76
2nd Series (Tests of Northumberland Type)						
Trial	432	100·4	14·6	·81	·76	·65
Examination	175	103·7	16·2	·95	·91	·77
3rd Series (Tests of Moray House Type)						
Trial	88	97·6	14·4	·78	·72	·58
Examination	126	102·1	15·3	·89	·85	·74
4th Series (Same Groups: Trials Before and After)						
Trials	283	100·2	16·1	·82	·78	·62
Examination	283	103·4	16·7	·94	·87	·75
5th Series (Same Groups: Monetary Reward)						
No Reward	131	96·8	16·5	·76	·68	·57
Reward	131	104·3	14·9	·97	·91	·54

TABLE II. RESULTS FROM STUDENTS

	Number	Average	S.D.	Reliability		Validity
1st Series (Students for Teacher's Diploma)				(*a*)	(*b*)	
Trial	317	123·0	11·4	·81	·78	·56
Examination	286	128·8	13·1	·93	·90	·69
2nd Series (Students taking Honours Degrees)						
Trial	148	129·6	12·6	·84	·76	·43
Examination	123	135·2	14·2	·91	·89	·58

a trial application of tests of the Moray House type, and an actual examination with these tests for children of 11 plus. Owing to the conditions under which the official examinations were necessarily arranged it was not possible to carry out any rigorous matching; but we believe the batches of children used for the ' trials ' were in every case of the same general level as the candidates who sat in the actual ' examinations '.

However, data are also available in which the *same* children sat both for the ' trials ' and for the ' examinations '. In this experiment (the ' fourth series ') the primary purpose of the research was to determine advantages or disadvantages of giving the candidates a trial run with the intelligence test before they actually sat for the examination. Eight groups took part. Groups A and B sat for the scholarship examination first, but were then required to take a second paper of similar type to determine the effect of familiarity unsupported by any special interest. To rule out any possible difference in the tests, Group A took booklet (*a*) first and (*b*) second, Group B took booklet (*b*) first and (*a*) second. Groups C and D took the preliminary paper *before* the scholarship examination; and, as before, one group took booklet (*a*) first, and the other booklet (*b*). With these latter groups the effects of the preliminary practice were presumably reinforced by motivation. The arrangements for the whole experiment were made with the help of the Chief Inspector of L.C.C. schools, Dr. C. W. Kimmins, and of the Chief Examiner, Dr. Daniels. An analysis of variance showed that in this case there was no significant difference either in the average ability of the different groups or in the average difficulty of the various sets of tests. Previous experience, however, appeared almost invariably to improve the children's average performance (though seldom to any large extent); and as a rule it significantly increased the reliability of the tests (though not their validity). But this is a matter that hardly concerns us here. In Table I therefore we have limited the figures to the average results (*a*) from the ' trials ' where there was no special motivation and (*b*) from the ' examinations ' where the motivation was strong.

In the main investigations the only material available for determining reliability consisted of a single set of tests taken at a single sitting; and the split-half method was used (column (*a*)). However, with smaller groups of children parallel tests of similar nature and difficulty were also applied at different sittings to determine reliability in the older and more informative way (column (*b*)). To estimate validity the scores obtained in the tests were correlated with gradings secured from teachers. An attempt was made to equate the gradings submitted by teachers at different schools by means of the average test-performances of the pupils in each school; but probably the figures so obtained were not very trustworthy.

The incentives aroused by an actual examination do not affect all candidates alike. We frequently noted that for many of the duller children who recognized that they stood no chance of success the examination seemed a sheer formality, and their attitude was one of careless boredom. Accordingly, a further experiment was arranged in an endeavour to provide a motive which should be much the same for all. In this—our 'fifth series'—the children were first given a trial set of tests. Six weeks later (in what for purposes of comparison might be called 'the examination') a second set of tests of a similar type was applied; but the children were told before commencing that any one who succeeded in beating his previous record by more than 15 marks—the equivalent of 3 I.Q. points (slightly over the amount to be expected as a result of the previous practice)—would receive a prize of threepence. For children in the rather poor district we were testing, this small reward, at the time of the experiment, constituted a fairly strong inducement.

Four batches, each consisting of 30 to 35 children aged 10½ to 11½—two groups of boys and two groups of girls from four different schools—took part. With the exception of five children (two girls who were slightly indisposed on the second occasion, and three boys who had probably made an undue proportion of lucky hits on the first occasion) everyone scored a higher mark in the second trial: twelve, however, (9 per cent) did not exceed their earlier scores by more than three. From the figures given in the table (means for all four groups) it will be seen that the average improvement was greater than in any of the other experiments; the reliability was also greatly increased; the validity, as judged by the teachers' ratings, was at two of the schools actually lower in the 'examination' than in the 'trial'; and the standard deviations were reduced at three schools and virtually unchanged in the fourth. The lowered validity seemed due to the fact that several moderately able children had on the second occasion (as one of the teachers expressed it) "put forward an effort which they have never been known to make in the class room before": (probably "specimens of habitual laziness", was the headmaster's comment).

Since the motivated series was necessarily the second series, it is not possible to separate the increment (if any) due to previous practice from that due to motivation. However, in pilot trials with the same tests but other groups the average improvement attributable to practice never exceeded 3 marks, and indeed was usually non-significant. Here, as the table shows, it amounts to 7·5 marks.

So far as our immediate problem is concerned, the final answer seems clear. In each of the five series the test-results obtained when strong motivation was operative showed (i) a higher average, (ii) a higher reliability, and usually (iii) a higher validity. This brief statement, however, somewhat oversimplifies the facts. In all such experiments there were appreciable differences between different schools—particularly between schools from different districts. In the 'trials' much depended on the disciplinary atmosphere habitually obtaining in the classroom, and the specific conditions under which the tests were administered. Except in those rarer instances where the teachers were sceptical or even hostile in regard to the introduction of psychological tests, the best results were nearly always secured when the children's own teacher administered and supervised the tests. When they were administered by a psychologist who was a member of the school inspectorate, or by a research student who was himself a teacher of considerable experience, the results were, as a rule, equally satisfactory. They varied widely, however, when the tests were administered by an outside research-student, particularly if he had no teaching experience or adopted a conspicuously unfamiliar approach (e.g. a student with marked overseas accent or a typical ' Oxford-and-Cambridge manner '); in such cases the reliability, validity, and level of performance were liable to drop to a remarkable degree. These, we believe, are factors which are too often overlooked in many of the published reports of researches in this field.

III. DATA FROM STUDENTS

In this country the earliest attempts to introduce tests of intelligence into entrance examinations for colleges or universities were, so far as we are aware, those carried out at the Institute of Education (formerly the London Day Training College) between 1920 and 1930. The Principal, Sir Percy Nunn, had taken part (with Dr. Daniels and the senior author) in the interviews and experimental tests carried out with junior county scholarship candidates; and he readily agreed to the introduction of both group and individual tests of intelligence for applicants for places at the Institute. One reason why such tests proved specially useful in this case was that the students at the Institute, who were trained for the postgraduate Teachers' Diploma Examinations, not only came from widely different schools and home backgrounds, but also took widely different subjects in Arts or Science for their preliminary degree at different colleges in the University.

The component tests were of much the same type as before, but of greater difficulty. The booklets used, generally given in roneo'd form, resembled those afterwards published by the National Institute of Industrial Psychology under the title ' Group Test 33 '. In order to validate and standardize the set of tests used in any particular year, the test-problems proposed were tried out on students of an earlier year—sometimes at the Institute of Education and sometimes (with the cooperation of Professor Spearman, Dr. Flugel and Dr. White) at University College. Later at University College similar tests were used for applicants for entrance to the Department of Psychology. In this case the trial experiments were chiefly carried out (with the assistance of Dr. Flugel, Dr. White, and Dr. William Stephenson) on volunteers from all departments.[1]

The relevant results are summarized in Table II. The figures given in the column headed validity are correlations based on ratings supplied later on by members of the staff (correlations with subsequent examination results will be found in the publications cited in the list of references). The correlations both for validity and for reliability refer to comparatively small batches; but the differences are still statistically significant. The conclusions to be drawn from the figures are the same as those already deduced from the data for children.

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[1] Tests along similar lines were carried out during the inter-war years at a number of different departments and colleges—e.g. by Dr. Barbara Dale at Newnham College, Cambridge, Dr. Victoria Hazlitt at Bedford College, London, by Dr. Dobson at Bristol, and Professor Vernon at Glasgow [5, 10]. Of these the investigations by Dr. Dale are most nearly comparable to our own, since she mainly relied on Group Test 33. Recently the testing has been resumed at University College by Dr. Charlotte Banks who has used Professor Valentine's newer group test [9]. Readers overseas who are unfamiliar with the conditions of entrance in this country may usefully refer to Dr. Furneaux's account [7]. An instructive review of the whole subject with a classified bibliography has been published by another former member of University College, Professor Eysenck [6].

IV. Summary and Conclusions

Group tests of intelligence have been applied to large numbers of school pupils at the ages of 10 and 11 and of students (or applicants for studentship) between the ages of 17 and 22. With both types of examinee comparable tests have been applied (*a*) under conditions involving no special motivation and (*b*) under conditions supplying a strong motivation—e.g. examination for entrance to a grammar school or a university department, and (in the case of children) a monetary reward. In practically every case it was found that, where a strong incentive operated, the mean of the performances was much higher and the reliability of the tests much greater. In most cases the validity of the tests was also improved; but here the increase was neither so large nor so consistent. The standard deviations increased in those examinations where the motivation was strongest in the case of the brighter candidates, and decreased in the experiments where it was equally strong for the duller examinees.

The nature, the mode of operation, and the effects of the motivation differ widely with different conditions and with different individuals; and the whole problem calls for further research, deliberately and systematically planned to deal with various specific issues. In the main, the variations due to the more specific differences seem comparatively slight; consequently fairly large groups and fairly elaborate experimental and statistical techniques will be needed to achieve conclusive results on these supplementary points. The most effective type of motivation appears to be a determination, or perhaps one should say an attitude, which is partly habitual and dependent on past experience, and partly conscious and results from a more or less explicit recognition that, if the individual tested puts forth the maximum effort of which he is capable, there will be an increased likelihood that he will secure in the near future some specific reward or satisfaction such as will recompense him for his trouble.

Two practical corollaries follow: first, that conclusions based on test-results cannot be accepted at their face-value unless the motivational conditions under which the test was applied are taken into consideration, and secondly, that test-results obtained under different motivational conditions cannot, as a rule, be validly compared.

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ABILITY AND ATTAINMENT: A NEW PSYCHOMETRIC FORMULATION OF THE CONCEPT OF EDUCATIONAL RETARDATION

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The following paper is concerned with the assessment of educational achievement and its relation to general ability. A new psychometric formulation of the problem is presented and its practical applications discussed. The main conclusion is that we should standardize available test scores in a new way. The attempt to formulate a model demonstrates that, although controversy has raged around the use of the achievement ratio, the theory of retardation and the logic of its numerical expression have never been made sufficiently explicit. It is shown that this has led to irrelevant and unnecessary arguments. A new description of the implications of retardation theory is proposed, and its relations to the psychometric model explained and illustrated.

I. Introduction

The assessment of children's educational achievements in relation to their general ability has occupied the attention of educational psychologists for some time.[1] For at least forty years much argument has centred on the use and meaning of the achievement ratio—the ratio of the attainment quotient or attainment age to the intelligence quotient or mental age. Since 1956 the arguments have been reiterated and developed in British journals in response to Pidgeon and Yates' comments on experimental data they had obtained concerning this ratio [11]. In an article published in 1960, Pidgeon concluded: " The important issues raised by these findings and by the subsequent discussion on the relationship between ability and attainment . . . are still not entirely resolved " ([10], pp. 124–5). Crane [4] has given a short historical account of the arguments down to 1958, listing 39 references which would be made more complete by the addition of Brimer [2, 3], Morris [6], Peel [7], N.F.E.R. [14, 15], Pidgeon and Yates [12], and Phillips [8].

Rather than repeat these arguments at the outset and allow ourselves to fall a prey to the usual mixture of assumptions, facts, opinions, and dogmas, the psychometric theory is here presented first of all with little theoretical comment. It is suggested that the whole controversy rests on inadequate notions of what the hypothesis of retardation implies in terms of the theory of mental testing and its numerical expression. This thesis is developed in Section

[1] The writer's interest in this field was initially stimulated by discussions with Mr. C. J. Phillips (see ' References ') to whom he is much indebted.

III where the retardation theory is set out in explicit detail. Further properties of the psychometric expression of this theory and their practical application will be discussed in Section IV.

II. PSYCHOMETRIC FORMULATION AND DERIVATIONS

The development in this section follows the method used by Gulliksen [5] in his theory of mental tests.

Definitions. Consider two scores x and y defined in the following way:

$$\left.\begin{aligned} x &= x' + e_x, \\ y &= y' + e_y - f', \end{aligned}\right\} \tag{1}$$

where x, an ability test score, is made up of a true score (x') and a random error component (e_x); and y is an attainment test score, made up of a true score (y') an eraor component (e_y) and a true retardation component (f'). The observed retardation, f, is found as

$$f = x - y. \tag{2}$$

The equations in (1) and (2) refer to an individual, the usual subscript i having been omitted. The means of all variables are put equal to zero for algebraic convenience. The standard deviations of the variables will be designated as σ_x, $\sigma_{x'}$, σ_{e_x}, and $\sigma_{f'}$ with analogous symbols for the remaining y components. No assumptions are made about the distributions of the variables except the usual one that the error components are random normal deviates. It will be assumed that all terms on the right hand side of the equations in (1) are uncorrelated with the exception that

$$x' = y'. \tag{3}$$

This implies that the true scores of x and y are perfectly correlated and have equal variances. Then

$$x - y = f = e_x - e_y + f', \tag{4}$$

which indicates the relation between the estimate of retardation (f) and the true retardation (f').

Derivations. From the equations in (1) we obtain

$$\left.\begin{aligned} \sigma_{x'}^2 &= \sigma_x^2 - \sigma_{e_x}^2. \\ \sigma_{y'}^2 &= \sigma_y^2 - \sigma_{e_y}^2 - \sigma_{f'}^2. \end{aligned}\right\} \tag{5}$$

Applying the definition in (3), the right hand sides of (5) are equal. Now the error variances may be defined in the usual way as

$$\left.\begin{aligned} \sigma_{e_x}^2 &= \sigma_x^2\,(1 - r_{xx}), \\ \sigma_{e_y}^2 &= \sigma_y^2\,(1 - r_{yy}), \end{aligned}\right\} \tag{6}$$

where r_{xx} and r_{yy} are the reliability coefficients for x and y respectively. After substitution and re-arrangement (5) becomes

$$\sigma_{f'}^2 = r_{yy}\sigma_y^2 - r_{xx}\sigma_x^2. \tag{7}$$

Squaring (4) we obtain:

$$\sigma_x^2 + \sigma_y^2 - 2r_{xy}\sigma_x\sigma_y = \sigma_{e_x}^2 + \sigma_{e_y}^2 + \sigma_{f'}^2. \tag{8}$$

Then, using the relations indicated in (6) and re-arranging terms, equation (8) reduces to

$$\sigma_{f'}^2 = r_{xx}\sigma_x^2 + r_{yy}\sigma_y^2 - 2r_{xy}\sigma_x\sigma_y. \tag{9}$$

Eliminating $\sigma_{f'}$ from eqn. (7) and (9) we obtain

$$\sigma_y = \sigma_x \frac{r_{xx}}{r_{xy}}. \tag{10}$$

Comments on the solution. Retardation, f, is obtained as the difference between x and y. These are scores having arbitrary means, but the standard deviation σ_y is determined from eqn. (10) as a function of three factors, namely, the arbitrarily chosen standard deviation of x, the reliability coefficient of test x, and the correlation of x and y. Thus for example, if we put $\sigma_x = 15$, $r_{xx} = 0{\cdot}9$ and $r =_{xy} 0{\cdot}6$, then $\sigma_y = 22{\cdot}5$. To the reader it may not be immediately obvious that anything is affected if score y has a standard deviation different from that of score x. Table I illustrates the effect. Three children having the same chronological age are shown under three different conditions of standardization of test scores. While the rank order of the three children is the same for general ability and for achievement with each condition, the decision as to which child is most seriously retarded will be different in each case. Less dramatic effects would be found if the intercorrelation of scores were less than perfect. Adding different constants to the first and second rows or multiplying both rows by the same constant does not affect the rank order of the children in the third row.

TABLE I. THE EFFECT OF DIFFERENT S.DS. OF SCORES x AND y ON THE ESTIMATE OF RETARDATION

Child	$\sigma_x > \sigma_y$[1]			$\sigma_x = \sigma_y$			$\sigma_x < \sigma_y$		
	A	B	C	A	B	C	A	B	C
General ability (x)	6	10	14	6	10	14	6	10	14
Attainment (y)	6	7	8	3	7	11	0	7	14
Retardation ($x-y$)	0	3	6	3	3	3	6	3	0

The variance of f is made up in the following way:

$$\sigma_f^2 = \sigma_{e_x}^2 + \sigma_{e_y}^2 + \sigma_{f'}^2. \tag{11}$$

[1] Under eqn. (10) the variance of x cannot, of course, be greater than the variance of y, since the reliability coefficient sets an upper limit to the correlation one variable may have with another. Table I merely demonstrates the effect of differing variances in general.

From the relations developed above, the components on the right hand side of (11) may be expressed in terms of σ_x, r_{xy}, r_{xx} and r_{yy}:

$$\left.\begin{aligned} \sigma_{e_x}^2 &= \sigma_x^2(1-r_{xx}),\\ \sigma_{e_y}^2 &= \sigma_x^2\left(\frac{r_{xx}}{r_{xy}}\right)^2(1-r_{yy}),\\ \sigma_{f'}^2 &= \sigma_x^2 r_{xx}\left[\frac{r_{xx}r_{yy}}{r_{xy}^2}-1\right]. \end{aligned}\right\} \tag{12}$$

The first two terms show the proportion of the variance of f which is due to error. Table II below gives illustrative values for the proportion of σ_f^2 which is true variance for different values of r_{xy} and r_{xx} putting $r_{xx}=r_{yy}$.

TABLE II. THE PROPORTION OF TRUE VARIANCE IN THE RETARDATION ESTIMATE FOR SELECTED VALUES OF r_{xy} AND r_{xx}, WHERE $r_{xx}=r_{yy}$

The figures in the body of the table represent percentages

	r_{xy}				
	0·5	0·6	0·7	0·8	0·9
r_{xx} and r_{yy}					
0·80	63·7	52·8	34·7	00·0	—[1]
0·85	73·4	65·5	52·1	25·6	—[1]
0·90	82·6	77·6	68·9	51·3	00·0
0·95	91·5	89·1	84·9	76·4	50·7
1·00	100·0	100·0	100·0	100·0	100·0

[1] These combinations or r_{xy} and r_{xx} are unreal.

The components of the variance of f are not, however, symmetrical in r_{xx} and r_{yy}: the proportion of error variance is more sensitive to changes in reliability of test y than it is to changes in the reliability of test x. This is illustrated in Table III for three levels of r_{xy}. Compare, for example, for following two conditions for $r_{xy}=0{\cdot}6$: (i) $r_{xx}=0{\cdot}8$; and, (ii) $r_{xx}=0{\cdot}8$ and r_{xy} 0·9. In (i) the proportion of true variance is 62·1 per cent ; in (ii) it is 67·9 per cent.

TABLE III. THE PROPORTION OF TRUE VARIANCE OF THE RETARDATION ESTIMATE FOR SELECTED VALUES OF r_{xy}, r_{xx} AND r_{xy}

The figures in the body of the table represent percentages

Reliabilities	$r_{xy}=0{\cdot}5$		$r_{xy}=0{\cdot}6$		$r_{xy}=0{\cdot}7$	
	$r_{xx}>r_{yy}$	$r_{xx}<r_{yy}$	$r_{xx}>r_{yy}$	$r_{xx}<r_{yy}$	$r_{xx}>r_{yy}$	$r_{xx}<r_{yy}$
0·9, 0·8	69·3	76·7	62·1	67·9	49·5	53·2
0·9, 0·7	56·1	68·2	46·6	54·6	30·1	33·3
0·8, 0·7	50·6	55·6	37·7	40·5	16·2	16·7

In summary, the major features of the model are: the inequality of the standard deviations of x and y; the demonstrable influence of the intercorrelation of x and y and their reliabilities on the error of measurement of f (the retardation score); and the unequal sensitivity of the error variance to changes in the reliabilities of x and y.

III. RETARDATION THEORY AND THE CURRENT CONTROVERSY

The theory of retardation and the source of the controversy must first be made explicit. The theory that has been called in question is that general ability prescribes an upper limit to scholastic achievement. Pidgeon and Yates write as follows: " Tests of ' intelligence ' or general ability, it has been held, provide a measure of an individual's capacity for scholastic achievement: tests of attainment indicate the level of skill and knowledge that an individual has acquired in a particular subject" ([11], p. 24). Brimer ([3], p. 25) summarizes the source of the argument: " [It has been] suggested that an intelligence test score does not establish the limit of functioning in any one attainment, since there were shown to be at least as many children whose attainment scores exceeded their intelligence scores on the tests used as there were children who showed the reverse characteristics ."

Pidgeon and Yates summarize their own standpoint by saying that " the real question at issue [is] whether or not ' over-achievers ' are few and rare " ([14], p. 33). Both the implications of the theory and the crux of the controversy can be seen quite clearly if they are approached in a simple manner. Let us first imagine that we are developing the numerical expression of retardation theory anew. It should be noted that a distinction has been made between ' numerical expression of retardation theory ' and ' retardation theory '. First of all, the concept of mental age is defined so that a mental age of (say) eight refers to the average score of eight-year-old children in what we call an intelligence test. Similarly the concept of attainment age is defined so that an attainment age of (say) ten refers to the average scholastic performance of ten-year-olds. Both these concepts are intuitively reasonable, although it must be admitted that this way of expressing scores obtained with tests is somewhat elaborate.

It is at this point that Pidgeon and Yates, like others before them, confound the theory by attacking what will be shown to be completely erroneous numerical expression of it. Giving a succinct summary of the theory which they attack and few competent psychologists have held, they say: " One would expect scores derived from two such tests for any given child to correspond closely, provided the tests have been designed to yield the same mean and standard deviation and provided also that the child concerned has been efficiently taught and has had an unhampered opportunity to profit from his instruction. It is reasonable to suppose that this latter condition is not invariably fulfilled, and to expect therefore to find some children whose scores in tests of attainment fall short of those that they obtain in a test of general ability or capacity. It is patently absurd to speak of achievement exceeding capacity and so one would expect to find few, if any, children obtaining scores in a test of attainment higher than those recorded in the test of general ability " ([11], p. 24).

Now, according to the theory, it will be shown that two contrasting situations are implied. First, there is the imaginary or idealized situation where children are efficiently taught and have 'unhampered opportunity to profit' from instruction: that is, they have no emotional, motivational and social problems affecting their scholastic achievement. This means that retardation, caused by a multiplicity of factors in the real world, would not exist, and we can also imagine that there would be no errors of measurement. The numerical expression of this concept would be that attainment age equals mental age, and that the ratio of these two numbers would be unity for all children. *The important point here is that in order to obtain this theoretical result we should have to imagine that the norms for mental age and for attainment age have been derived from experiments on children belonging to an ideal educational world.*

If we now carry out our experiments in the *real* world of education, and derive our norms from real children, then the retardation theory we are developing here would suggest that, while the mental ages (or equivalent normative scores) remain the same, children will in general be educationally retarded. Here lie the critical points of the controversy. Two points have to be considered:—

(1) that children are in general educationally retarded;
(2) that mental ages (or the equivalent normative scores) are unaffected by the factors producing retardation.

(1) The former proposition indicates the starting point of the controversy, though in a more explicit form. If the children of the real educational world are in general retarded because of their imperfect psychological and educational environment, then the normative data with which we construct attainment ages will be depressed by this retardation below the ideal scholastic achievement. However, it is impossible to gauge how much the whole population of school-children is retarded, because no experiment we could ever devise would show us what the standards for ideal attainments are. As Morris pointed out quite early in the controversy: "There are no 'natural' standards" ([6], p. 35). All we can do is estimate lower bounds for these standards. Attainment ages obtained with current tests are merely our present estimates of the lower bounds.[1]

Now, while an attainment age remains meaningful to the teacher as a statement of a child's performance in relation to his contemporaries', the comparison with mental age has changed its point of reference. The comparison that the achievement ratio makes is between general ability (its mean set arbitrarily at 100) and attainment, to which we have also given a mean of 100 despite the fact that attainment is depressed by retarding factors. Thus, differences of zero, or ratios of unity, do not have the meaning attached to them by the practitioners of the theory and by Pidgeon and Yates and other critics. A mean of the difference between quotients or ages is, therefore, a purely statistical construct.

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[1] Alternatively, we could express this argument in terms of variation in level of motivation so that the norms for attainment are based on *average* motivation and not on *maximum* motivation.

Consider a child who is ten years of age and has a mental age of twelve and an attainment age of thirteen. Retardation theory, as expressed above, would suggest, not that this child is advanced, accelerated, or over-achieving, but that thirteen year olds are on the average *at least* one year retarded at this point in time. In practice of course, because of errors of measurement and other considerations, it would be unsatisfactory to estimate the retardation of a group on the basis of information about one child; but the underlying argument, which is merely illustrated here, is believed to be sound. The achievement ratio *underestimates* a child's retardation.

In view of these considerations, the controversy over the form of the distribution of the ratios, its normality or non-normality, its symmetry or asymmetry and the proportions above or below the mean, is completely irrelevant to the proof or disproof of the validity of the theory.[1] These matters are purely theoretical statistical questions, and have no reference to educational psychology as such. Given that two scores are jointly distributed in a particular way with given means and standard deviations (and we create these things more or less arbitrarily in psychology), the important features of the distribution of their ratios, or their differences, may accordingly be determined without recourse to arithmetical operations on experimental data. To put it bluntly, a statistician could have told us forty years ago that if we equalize the means and standard deviations of two distributions where the correlation is less than perfect, then a considerable proportion of the differences would be greater than zero and that an analogous result would obtain for the ratios. Finally, since, as we have said, there is no way of estimating the degree of retardation of a population of children as a whole, we have no condition, either in theory or in its numerical presentation, for specifying the concept of 'over-achievement'. Similarly, the term 'acceleration' might best be limited to its use in the clinical field to describe the favourable progress of a severely retarded child.

(2) As the controversy raged in the N.F.E.R. bulletins most of the contributors concerned themselves with the erroneous argument that so-called 'over-achievers' were similar in number to the under-achievers, or that there were more 'over-achievers' than they had anticipated. Some were stimulated to explain away 'over-achievement' in devious ways. The most serious attack, however, was levelled against the nature of intelligence test scores. Expressed in terms of retardation theory, what the critics questioned was the notion that mental ages (or the equivalent test scores) are unaffected by the factors causing retardation. Evidence and views on this subject would fill many books. Consequently, a brief and rather practical view will be expressed which will serve our present purpose.

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[1] Phillips [8] wrote: 'It seems to me that the symmetry question has but marginal relevance to the problem.'

We can consider that an intelligence test score is a short term *estimate* of how well a child is likely to perform new tasks. There is no need to regard this score as representing an accurate measurement of innate general ability alone, nor indeed need we regard it as anything but influenced by environmental factors. All we require is an assurance that an intelligence estimate has a stability which is greater than that of the achievement estimate. In other words, we only need to be assured that, while level of attainment is sensitive to recent and current environmental influences, the level of intelligence is less sensitive. Thus, all that retardation theory demands is that intelligence scores are *relatively* free from retardation factors—relative that is, to achievement scores. A final consideration under this heading is, that in view of the revised psychometric formulation of the theory and the arguments under (1), it is unnecessary to call into question the nature of intelligence scores until we have exploited and tested the concepts of our present theory to the full.

A Brief Re-Statement. The theory that we have been formulating is best expressed by the psychometric model of Section II. However, a verbal description may be given with the relevant psychometric definitions.

General ability (x) shows the capacity for scholastic achievement (y). Ideally, scholastic achievement reaches capacity ($x'=y'$; $r_{x'y'}=1$; $\sigma_{x'}=\sigma_{y'}$). In practice, however, this relation is reduced to r_{xy} owing, on the one hand, to errors of measurement (e_x, e_y; r_{xx}, r_{yy}), and on the other to a multiplicity of complex factors which result in retardation (f'). Retardation is estimated as the discrepancy (f) between ability and actual scholastic achievement. It follows that our estimate of the degree of retardation is a function not only of true retardation, but also of the errors of measurement. The variation between children due to general ability (σ_x^2) is less than that shown by the measure of scholastic achievement (σ_y^2) because children also vary in their degree of retardation ($\sigma_{f'}^2$). Finally, a practical point is that measures of ability should be chosen so that the psychological functions they tap are, relatively speaking, more stable than scholastic achievement. This is a *sufficient* requirement.

IV. THE PSYCHOMETRIC MODEL

The three major features of the model discussed so far are: (a) that the variance of the attainment scores should not, in general, be equated with the variance of the intelligence scores (eqn. (10) and Table I); (b) that the relation between the variances of the retardation scores and errors of measurement is made explicit (eqn. (11) and Table II); and (c) that the estimate of retardation is not equally sensitive to the errors of measurement of the two scores (eqn. (12) and Table III).

No assumptions have been made about the distributions of x, y and f and are unnecessary to the original formulation. In practice, however, x and y are developed so as to approximate to the normal distribution. For convenience of interpretation it is useful to have a linear relation between x and y, and to postulate the properties of normality and equal variances within the arrays. Given this system of variables, the distribution of f is prescribed.[1] There is no theoretical reason why we should require that the distributions of f and y to be normal; but when any appreciable amount of error variance is present, it is difficult to produce distributions that do not tail away in both directions. The main point is that the theory does not specify in what way retardation affects the distribution of y, and reference to experimental data does not help here because item analysis and prior assumption have already determined the distributions found in such data.

Practical Application of the Model. By statistical definition, not by psychological assumption, the mean of f is zero in the present model; in practice it may be whatever value we care to make it. This brings us to the question how the model may be developed and expressed in practice. First, the data for each age group would have to be available so that the necessary scores and their correlation and reliability coefficients could be derived from the one body of data. Secondly, the scores would be given arbitrary means, but the ratio of their standard deviations would be determined in accordance with the model. Thirdly, the mean and variance of the retardation scores would be determined by the previous operations, leaving only the problem of how the retardation estimate might best be presented to teachers and remedial workers. It is suggested that the distribution of retardation scores should be presented in percentile form, so that a rank of (say) 90 represents relatively low retardation, i.e. high efficiency.

This admittedly is a greatly simplified method of assessment, for, as Burt [1] has suggested, the practice of streaming children, and upgrading them according to some complex compound of age and ability, may aggravate retardation at particular levels of ability in relation to other levels.[2] No psychometric model can decide which kind of retardation—that which obtains at high levels or that which obtains at low levels—presents the more serious educational problem.

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[1] See, for example, Burt ([1], pp. 16–17).

[2] *Editorial Note.* Professor Burt writes: " I am not sure that this way of stating my views really brings out the point I was trying to make in the note Dr. Levy cited. As he observes, the norm for attainments is an average. It must therefore indicate a level *below* what a child of average ability *could* do, since nobody's achievements come up to the maximum which his actual ability would permit. Moreover, the average of a group represents a *medium* performance, and the individuals composing the group will deviate above and below the 'norm' which that average specifies. Since general ability is by no means the only factor determining these deviations, some will achieve more than we should predict from their general ability, and others less. Motivational factors are not the only additional influences; but, as laziness is commoner than exceptional industry, motivational factors are more likely to depress attainments than to raise them. Equally important are the educative conditions in the home, and above all those special abilities which bear on work in the class room. I doubt whether the practice of streaming or the method of promotion is likely to *aggravate* retardation. On the contrary, according to my own data the variance for attainments is usually less than the variance for ability. And finally, I have argued that ' the problem of the achievement ratio ' need no longer remain to perplex us, once we realise that the achievement *ratio* is an inappropriate measure; the proper measure is the *difference*. These, however, are minor points. I fully agree with most of Dr. Levy's highly interesting discussion ."
S.P.

For such a decision social criteria, not psychometric criteria, are needed. What the model can do is to reveal which children are the most gravely retarded at each age and within each level of ability. It is then a matter of judgement how many children at each level should receive remedial treatment. This has always been a problem with the achievement ratio, and will remain so, no matter what psychometric model we adopt. Thus, there is no escape from the need for some such model to be used and interpreted with the aid of some formulation of social utility. In the recent controversy this point was somewhat obscured by the statistical artefacts produced by using the achievement ratio with an improper development.

An important application of the measurement of retardation is the assessment of change under remedial training[1]. In the model presented here change of percentile rank as the child passes from one age group to the next would appear to offer a criterion. The percentile ranking of a group of children, can claim no greater permanency than the present 'months of retardation'; but it avoids the suggestion that the measure of retardation can be absolute. The model also recognizes changing standards and changing 'efficiencies' at each age level. As the correlation between ability and achievement increases for fixed reliabilities in a particular age group, so the measure of retardation becomes saturated with error variance. An alternative way of expressing this is to say that when the correlation is high, retardation is no longer a serious educational problem. These concepts are illustrated in Tables II and III. If, in a cross-sectional study, the correlation coefficient is shown to diminish with increase of age, then either educational retardation is a real problem, or the general ability test is failing to measure an increasing number of special abilities. In the latter case, a multivariate interpretation of the score x would seem appropriate. Thus the advantage sometimes suggested for individual tests like the Stanford-Binet and WISC over group tests could be due, not so much to their individual nature, but to the mixture of items they contain. Finally, it should be remarked that the basic assumption underlying both the retardation score described here and the achievement ratio is that retardation is represented by all that variation in the attainment scores which is not predictable from ability scores. Two important consequences ensue: (*a*) that we should always be aware of the extent to which retardation scores may be affected by error variance; (*b*) that the present practice of comparing only one ability score with attainment tends to cause our assessments of educational retardation to be exaggerated.

V. CONCLUSIONS

The psychometric model presented here serves three purposes: it forces us to make the theory of retardation explicit and examine its assumptions; it dissolves the controversy that has surrounded the use of the achievement ratio;

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[1] See Pringle and Sutcliffe ([13], pp. 10–11) on the difficulties of assessing the changes produced by remedial training.

and it yields a rationally based numerical expression of the theory. There remains the problem of showing that the underlying concepts of the theory and its implied numerical operations are meaningful in practice. This is particularly important in the remedial field, since the theory does not suggest how retardation comes about, nor how it might be remedied. All that has been offered is a criterion in the form of a rationally developed metric.

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LATENT STRUCTURE AND POSITIVE MANIFOLD

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Analytic discrete class latent structure solutions must use third order joint positive frequencies and often cannot get all latent probabilities between zero and one inclusive. An early geometric approach gave a solution, permissible in that sense, but cumbersome because of complex rotational limits and centring criteria. The inclusion, after factoring, of a vector for each negative response makes the rotational limits those of a strictly positive orthogonal manifold, and the centring criteria those of a best-fitting orthogonal reference frame. After factoring but before rotation it can be determined whether a permissible r-class solution exists. An empirical example is provided, and various rotational approaches are discussed.

I. Introduction

In the early approximate solution [5, 6, or 8] to the general discrete class case of Lazarsfeld's latent structure analysis [17], graphic rotational techniques similar to those of factor analysis were used. The rotational objective was to satisfy certain important inequalities, namely, that the latent parameters, being proportions or probabilities, must all lie between zero and one, inclusive. More recently several analytic general discrete class solutions [1, 7, 12, 18] have been developed. These resolve the rotational problem or its equivalent in another way, namely, by the use of higher-order data and equations. However, since they do not make explicit use of the above-mentioned inequalities, they have yielded, more often than desired, inadmissable latent parameters [2]. Thus the geometric approach is not fully obsolete; and this paper will modify the rotational criteria as an aid in achieving acceptable general discrete class solutions.

II. Preliminary Development

The only equations needed to show how the inequalities mentioned can be brought to bear are the following:

$$\begin{aligned} 1 &= v_{\mathrm{I}}+v_{\mathrm{II}}+\dots+v_r, \\ p_j &= v_{\mathrm{I}}p_{\mathrm{I}j}+v_{\mathrm{II}}p_{\mathrm{II}j}+\dots+v_r p_{rj}, \\ p_{jk} &= v_{\mathrm{I}}p_{\mathrm{I}j}p_{\mathrm{I}k}+v_{\mathrm{II}}p_{\mathrm{II}j}p_{\mathrm{II}k}+\dots+v_r p_{rj}p_{rk}. \end{aligned} \tag{1}$$

These are the discrete class latent structure equations of zero, first, and second order ([17], p. 385). On the left are manifest data of those orders, p_j being the manifest marginal for item j, i.e. the proportion of the entire sample passing or endorsing or giving the so-called positive response to the jth item of a test or questionnaire, and p_{jk} being the proportion of joint positive response to items j and k. On the right are the latent parameters, the v's being the relative or proportionate sizes of the r latent classes, and the p's being the latent marginals, or the proportions within those classes responding positively to the items. The first two lines of eqns. (1) are valid for any method of classifying sample members into any number of mutually exclusive and exhaustive categories. The third line holds, however, only on the definition of a latent class as being homogeneous enough, with respect to whatever and however many underlying dimensions are involved, so that all within-class covariation among items is partialed out. This so-called local independence definition of a latent class allows the within-class proportions of joint positive response to be written as products of the separate proportions ([17], pp. 367–369).

Equations (1) may be stated in matrix terms[1] ([17], pp. 386–388) as

$$P=L'\,VL. \tag{2}$$

Here P is the symmetric matrix of manifest joint positive frequencies p_{jk}, bordered at top and left by row and column 0 containing the manifest marginal frequencies p_j, and with $p_o=1$. Row j of the rectangular matrix L' contains the latent marginals for item j, and row 0 contains only unities. V is a diagonal matrix with the relative class sizes as its elements. For n items and r latent classes, P is $(n+1)$ by $(n+1)$, L' is $(n+1)$ by r, and V is r by r.

The equation

$$F_L\equiv L'V^{1/2} \tag{3}$$

defines an orthogonal factorization F_L of P, i.e.,

$$P=F_LF_L'. \tag{4}$$

Here $V^{1/2}$ is a diagonal matrix containing the square roots of the class sizes.

Now the form of eqn. (3) indicates that F_L is proportional by columns to L', the proportionality constants being the diagonals of $V^{1/2}$. Thus, since L' contains only positive proportions no greater than the unities in its first row (row 0), F_L must have the property that each column has all positive entries no greater than its first entry. Geometrically this amounts to the requirement that the plot of any pair of columns of F_L must have all points within the rectangle whose diagonal is the line from the origin O to the point 0, and two of whose sides are the coordinate axes. This geometric contraint is shown in Figure 1 for axes I and II of a fictitious F_L. The axes I and II are in horizontal and vertical positions respectively, and item points 1–4 lie within the rectangle mentioned. Two other sets of axes and two other rectangles are also shown in Figure 1. The orthogonal reference frame I″–II″ is determined by point 1, and represents the counter-clockwise rotational limit of any permissible solution for latent classes I and II. Further counter-clockwise rotation would cause point 1 to project negatively on axis II, thus implying a negative latent marginal for item 1 in latent class II. The orthogonal frame I′–II′ is the clockwise rotational limit, and is determined by the possibility of point 2 projecting more highly on axis I than does the 0 point, thus giving a latent marginal exceeding unity for item 2 in class I. Note the double role of point 4. Almost simultaneously with the effect of points 1 and 2 in determining the rotational limits, it restricts rotational freedom in both directions because of the danger that item 4 could acquire either a negative latent marginal in class I or a latent marginal greater than unity in class II. Point 3 plays little role in restricting rotational freedom, at least in this plane. It is close to the diagonal from the origin to the 0 point; and this corresponds to a relatively flat profile of latent marginals for item 3 across classes I and II. The degree of rotational constraint shown in Figure 1 is not atypical of empirical experience.

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[1] British readers to whom the terminology here used may be unfamiliar will find it helpful to observe that the notation adopted in this section is identical with that proposed in Burt's *Factors of the Mind* 1940 (cf. equations on pp. 166 and 490) and adopted in earlier papers in this *Journal*.

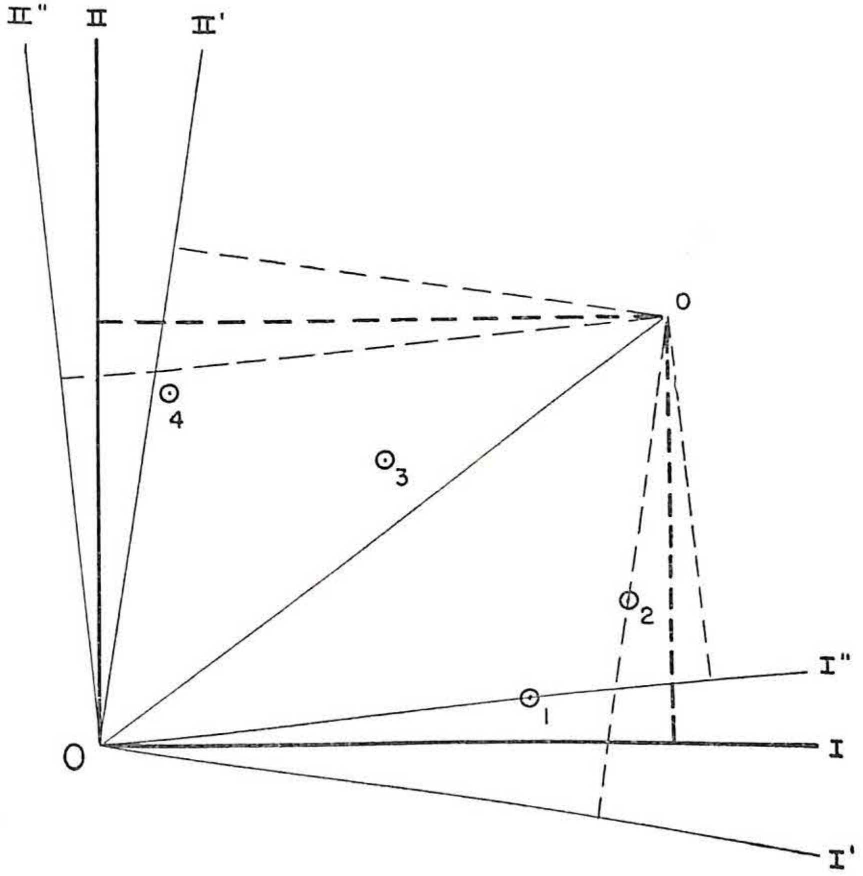

FIGURE 1. Plot of axes I and II of a fictitious F_L, showing clockwise (I′–II′) and counterclockwise (I″–II″) rotational limits.

The geometric considerations of the previous paragraph formed the basis for the approximate general discrete class solution [5, 6, or 8] mentioned earlier.

There the programme of solution was to obtain an orthogonal factorization F_A of P such that the equation

$$P = F_A F_A' \tag{5}$$

held good with negligible discrepancies; then by graphic means to rotate F_A orthogonally to a position roughly centered within the allowable limits. By 'roughly centered' was meant that the final set of plots should tend toward the following properties: (*a*) on each plot, approximate bisection of the angle between the rotational limits by the chosen reference axes; (*b*) across all plots, a fair degree of uniformity of the angle between the rotational limits; (*c*) on all plots, determination of the approximate position of each rotational limit by more than one point; (*d*) on each plot, determination of the rotational limits in such fashion as to be relatively unaffected by possible moves on other plots. The purpose of the present paper can now be stated: it is to convert the geometric constraints of the previous paragraph into those of a strictly positive orthogonal manifold, or of an orthogonal reference frame on which all vectors project non-negatively, in which case the centring requirements just discussed become those for a best-fitting orthogonal reference frame.

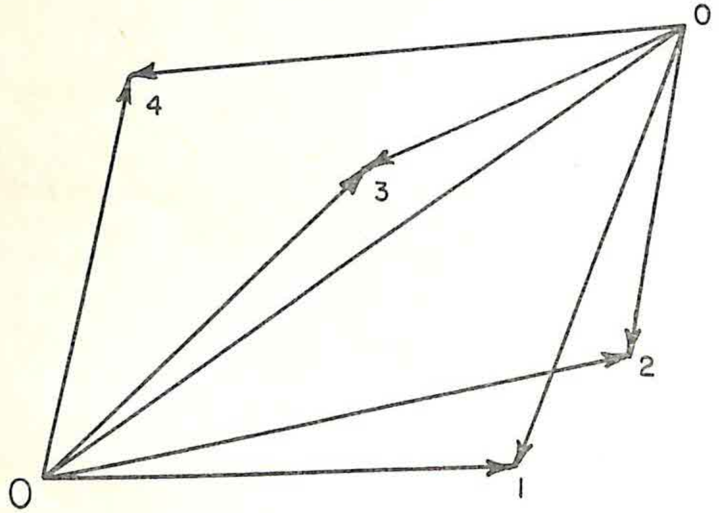

FIGURE 2. Vector configuration of Figure 1, with vectors drawn both from the origin O and the point 0 to all item points.

III. BASIC CONVERSION

Now consider Figure 2, showing the same set of points (including the origin O) as Figure 1, but with reference frames removed so as to emphasize that the problem with empirical data is to *find* a suitable reference frame. In Figure 2 vectors have been drawn from the origin to all item points. This is the type of origin-bound vector configuration customarily dealt with in factor analysis.

Figure 2 also shows vectors radiating from the 0 point to all item points. Here the principal modification is to take this second bundle of vectors, translate it to the origin, and then reflect every member, with the result shown in Figure 3. There the original set of four origin-bound vectors of Figure 2 is augmented by the translated and reflected set just mentioned. The latter is labelled with minus signs, for a reason that will soon become apparent. Absent from Figure 3 are the point 0 and the line from the origin to it; present in Figure 3 are clockwise (I′–II′) and counter-clockwise (I″–II″) rotational limits imposed by the requirement of strictly positive orthogonal manifold. Comparison of Figures 3 and 1 verifies that the two sets of rotational limits are identical.

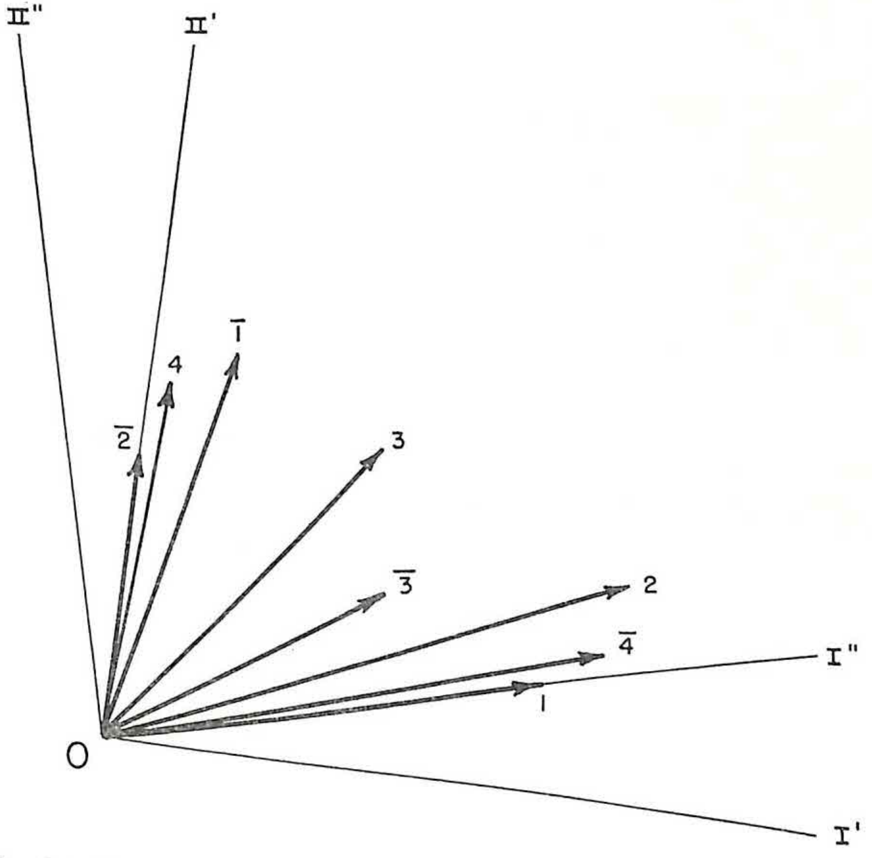

FIGURE 3. Modified vector configuration of Figure 2, with the 0 point bundle translated to the origin and reflected.

IV. CONVERSION MECHANICS

The mechanics of the foregoing translation and reflection are quite simple, regardless of the initial reference frame that is represented by F_A. The translation is accomplished by subtracting row 0 of F_A from every other row. The resulting second set of vectors is then reflected by simply changing signs. It happens that P is often factored in a special way, owing to the fact that its first diagonal, p_o, unlike later diagonals, is given. For this reason the first factor can easily be obtained by diagonal factoring ([21], pp. 101–105) pivoted on its row and column 0. Since $p_o = 1$, these first factor loadings become simply the manifest marginals p_j. Row 0 of F_A becomes unity followed by $(r-1)$ zeros, regardless of how subsequent factors are extracted. Thus in the translation of coordinates just discussed, only the first column of F_A is actually altered: p_j is converted into $(p_j - 1)$ or into $-q_j$, where q_j is the manifest proportion of *negative* response to item j. Then, reversing all signs for the reflection makes the second set of origin-bound vectors identical with what would result from including, *after* P is factored, a vector for the negative response to each item. The loadings of the negative vector $\bar{j}$ are, for the first factor, q_j, and for each remaining factor, minus the loading for positive vector j.

V. SOME THEORETICAL AND PRACTICAL BY-PRODUCTS

One important by-product of the preceding argument, already noted [5, 6, or 8], but perhaps not yet fully recognized, concerns a special relation between the discrete class latent structure case and factor analysis. Removal of the first factor from P by diagonal factoring puts the first factor residuals, P_1, into special form. Since the first factor loadings of vectors j and k are p_j and p_k, the first factor residual p_{1jk} is

$$p_{1jk} = p_{jk} - p_j p_k. \tag{6}$$

But the right-hand member of eqn. (6) is merely the numerator of the fourfold point correlation, ϕ_{jk}, between the j and k dichotomies ([14], pp. 339–342). It is in fact the covariance of those two dichotomies, convertible into ϕ_{jk} by dividing through both rows and columns of the residual matrix P_1 by the square roots of the associated variances, $p_j q_j$, $p_k q_k$, etc. Now this dividing through is equivalent to pre- and post-multiplication of P_1 by a diagonal matrix of reciprocals; geometrically its only effect is to change the lengths of the vectors of P_1 without changing their angular separations. Hence the vector space of P_1 is essentially the same as that of the correlation matrix for the same items. It follows that, if the missing diagonals for the two matrices are analogously estimated, and if comparable criteria of fit are applied, the number of latent classes needed to account for P is one more than the number of linear factors needed to account for the correlations.

An important theoretical and practical corollary of the last two paragraphs concerns means for determining in advance whether a permissible r-class solution is possible for a particular set of data. The scalar product, $p_{j\bar{k}}$, between the positive vector j and the negative vector $\bar{k}$ is expressible in terms of their first factor loadings, p_j and q_k, and their first factor residual, $-p_{1jk}$, as

$$\begin{aligned} p_{j\bar{k}} &= p_j q_k + (-p_{1jk}) \\ &= p_j q_k - p_{1jk} \\ &= p_j q_k - (p_{jk} - p_j p_k) \\ &= p_j q_k + p_j p_k - p_{jk} \\ &= p_j (q_k + p_k) - p_{jk} \\ &= p_j - p_{jk}. \end{aligned} \tag{7}$$

Now all scalar products among item vectors, whether positive or negative, must be non-negative in order that a strictly positive orthogonal manifold may exist. No vector may be more than ninety degrees away from any other: that is the definition of an orthogonal positive manifold. The simple form to which eqn. (7) reduces assures such a condition when P is fully factored, for the manifest marginal for either the positive or the negative response to item j will never be less than any joint proportion involving that response. However, there is a question whether an orthogonal factorization of only r columns will reproduce *all* joint frequencies, among both positive and negative responses, non-negatively. If so, an orthogonal positive manifold (and hence a permissible r-class solution) is possible; otherwise not. This can be known before any rotational efforts are made, or before an analytic r-class solution is attempted. Formerly, when an r-dimensional rotational process would appear to be unable to reach a positive manifold, it was uncertain whether this was only a seeming cul-de-sac, or whether an r-class solution was really impossible. When the analytic approaches fail to yield permissible r-class solutions, the considerations of this paragraph permit us to determine whether too small a value has been chosen for r, or whether the higher-order data that are used violate the local independence requirements of the model.

VI. An Empirical Example

This is a suitable point at which to introduce concreteness by considering an empirical example. At the top left in Table I is shown an earlier factorization F_A of a joint proportions matrix P, based on a sample of 793 American college students, for a set of nine attitude items. The first column of F_A consists of unity followed by the manifest marginals p_j for the nine items. Since this first factor was obtained by placing the first reference axis co-linear with vector 0, the projections of that vector on the remaining two reference axes are zero. At the top right in Table I is shown an earlier orthogonal rotation, F_B, graphically deduced, of F_A. The corresponding orthogonal transformation Λ_{AB} is shown at the top right in Table II. The reader might wish to plot the three pairs of columns of F_B as in Figure 1, to see how well centred the rotated orthogonal frame is for this example. An approximate latent structure solution was previously obtained by treating F_B as F_L, dividing its columns by the corresponding 0 row entry to get the associated matrix of latent marginals L', and squaring the 0 row entries of F_B to get the associated relative class sizes.

Below F_A in Table I is shown the matrix $F_{\bar{A}}$ giving the projections of the negative vectors on the reference frame of F_A. Each row of $F_{\bar{A}}$ is obtained by subtracting, from the corresponding row of F_A, the O row thereof, then reversing signs. Thus the last two columns of $F_{\bar{A}}$ are identical, except for sign, with those of F_A, while each entry in the first column of $F_{\bar{A}}$ is simply one minus the corresponding entry in F_A. Below F_B in Table I is shown the matrix $F_{\bar{B}}$, obtained by applying the orthogonal transformation Λ_{AB} to $F_{\bar{A}}$. $F_{\bar{B}}$ gives the projections of the negative vectors on the orthogonal frame of F_B. After plotting F_B let the reader add the points in $F_{\bar{B}}$; he will then find that the rotational limits for a strictly positive orthogonal manifold for both F_B and $F_{\bar{B}}$ are identical with those constructed as in Figure 1. It will be found that each of the four centring principles mentioned above readily converts to such a positive manifold. In short, those principles constitute a fairly unambiguous definition of best-fitting orthogonal positive manifold.

TABLE I. AN EMPIRICAL THREE-CLASS LATENT STRUCTURE EXAMPLE
(Data from [5], pp. 147–156, or [6], pp. 121–132)

	F_A				F_C				F_B		
	A	B	C		I	II	III		I	II	III
0	1·000	·000	·000	0	·641	·464	·612	0	·718	·442	·538
1	·201	·142	−·067	1	·039	·222	·120	1	·252	·215	·127
2	·678	−·270	·171	2	·596	·042	·453	2	·658	·030	·359
3	·298	·297	−·062	3	−·014	·356	·232	3	·014	·340	·235
4	·412	·282	·055	4	·045	·324	·382	4	·096	·300	·391
5	·092	·070	·035	5	·000	·062	·104	5	·015	·055	·107
6	·496	·199	·226	6	·122	·201	·530	6	·198	·170	·518
7	·354	·096	·135	7	·127	·133	·345	7	·176	·114	·330
8	·552	·345	·069	8	·085	·417	·497	8	·152	·386	·506
9	·375	−·066	·131	9	·260	·049	·303	9	·303	·037	·263

	$F_{\bar{A}}$				$F_{\bar{C}}$				$F_{\bar{B}}$		
	A	B	C		I	II	III		I	II	III
$\bar{0}$	·000	·000	·000	$\bar{0}$	·000	·000	·000	$\bar{0}$	·000	·000	·000
$\bar{1}$	·799	−·142	·067	$\bar{1}$	·602	·242	·492	$\bar{1}$	·666	·227	·411
$\bar{2}$	·322	·270	−·171	$\bar{2}$	·045	·422	·159	$\bar{2}$	·060	·412	·179
$\bar{3}$	·702	−·297	·062	$\bar{3}$	·655	·108	·380	$\bar{3}$	·704	·102	·283
$\bar{4}$	·588	−·282	−·055	$\bar{4}$	·596	·140	·230	$\bar{4}$	·622	·142	·147
$\bar{5}$	·908	−·070	−·035	$\bar{5}$	·641	·402	·508	$\bar{5}$	·703	·387	·431
$\bar{6}$	·504	−·199	−·226	$\bar{6}$	·519	·263	·082	$\bar{6}$	·520	·272	·020
$\bar{7}$	·646	−·096	−·135	$\bar{7}$	·514	·331	·267	$\bar{7}$	·542	·328	·208
$\bar{8}$	·448	−·345	−·069	$\bar{8}$	·556	·047	·115	$\bar{8}$	·566	·056	·032
$\bar{9}$	·625	·066	−·131	$\bar{9}$	·381	·415	·309	$\bar{9}$	·415	·405	·275

TABLE II

TRANSFORMATIONS FOR AN EMPIRICAL THREE-CLASS LATENT STRUCTURE EXAMPLE

	S_{AC}				Λ_{AC}				Λ_{AB}		
	I	II	III		I	II	III		I	II	III
A	·787	·709	·854	A	·641	·464	·612	A	·718	·442	·538
B	−·606	·595	·343	B	−·736	·597	·318	B	−·691	·554	·465
C	−·121	−·377	·389	C	−·218	−·654	·724	C	−·091	−·705	·703

	$S'_{AC}\ \Lambda_{AC}$				$\Lambda'_{AC}\ \Lambda_{AB}$		
	I	II	III		I	II	III
I	·977	·083	·201	I	·989	·029	−·151
II	·099	·931	·350	II	−·020	·997	·067
III	·210	·347	·913	III	·154	−·064	·986

There are several methods of using the positive manifold requirement to achieve the rotational objective represented by F_B. One has been applied to the present example, and is summarized in the remaining parts of Table I and Table II. The 18×18 matrix of scalar products among all item vectors (both positive and negative) was formed from the matrix products $F_A F_A'$. $F_A F_A'$, $F_A F_{\bar{A}}' = (F_{\bar{A}} F_A')'$, and $F_{\bar{A}} F_{\bar{A}}'$, eliminating O and $\bar{O}$ rows and columns. This is the matrix of reproduced joint frequencies whose non-negativeness indicates the existence of a permissible r-class solution. This was normalized by dividing each row and column by the square root of the associated diagonal. The result was a symmetric 18×18 matrix of scalar products among unit-length vectors colinear with the original 18 item vectors. These products were then scanned to discover the most purely diagonal 3×3 symmetric sub-section, the purpose being to isolate a set of vectors close to the corners of the positive manifold. The symmetric sub-section involving the positive vector 6 and the negative vectors $\bar{2}$ and $\bar{8}$ was thus found to have the lowest sum of side entries. After being normalized, reversed in order, and transposed, the three corresponding rows of F_A and $F_{\bar{A}}$ constitute the oblique transformation S_{AC} that is shown at the top left in Table II. The next step is to orthogonalize the oblique transformation S_{AC}, i.e., to obtain an orthogonal transformation closely related, axis by axis, to S_{AC}. Successive approximation was used as described in [10], though least-squares orthogonalization procedures (cf. [11] and [13]) are also available. The resulting transformation Λ_{AC} is shown beside S_{AC} in Table II. The matrix product $S_{AC}'\Lambda_{AC}$, giving the cosines between the oblique and the associated orthogonal axes, is shown at the bottom left. The degree of diagonality here indicates the one-to-one closeness of the oblique and orthogonal axes.

The matrix products $F_C = F_A \Lambda_{AC}$ and $F_{\bar{C}} = F_{\bar{A}} \Lambda_{AC}$ are shown down the middle of Table I. Inspection of F_C and $F_{\bar{C}}$ reveals only one violation of the requirement for a strictly positive orthogonal manifold—the negative projection of positive vector 3 on axis I. This violation also appears in the fact that the negative vector $\bar{3}$ projects more highly on axis I than the 0 vector of F_C. The matrix product $\Lambda_{AC}' \Lambda_{AB}$, giving the cosines of angles between the orthogonal axes of F_C and the centred orthogonal frame of F_B is shown at the bottom right in Table II. Once again the diagonal form indicates a high degree of one-to-one closeness. Further small adjustments of F_C and $F_{\bar{C}}$ would eliminate the last violation of positive manifold and centre the reference frame within the allowable limits.

VII. OTHER CORNER VECTOR APPROACHES

There are other ways to isolate, in advance of any rotation, a set of item vectors close to the corners of the positive manifold. One would be to use the extended vectors plots ([21], pp. 225–230) of the combined F_A and $F_{\bar{A}}$ in a manner differing not at all geometrically from the so-called direct rotation procedure of Harris [15]. The extended vectors plot of axis III against axis II of F_A and $F_{\bar{A}}$, obtained by plotting those two columns *after* each row, was divided through by its first entry, and might well have suggested the same three vectors as were isolated in the previous paragraph. The triangle formed by these three points on that plot encloses as many of the other points as any other such triangle. The same thing is apparent in the raw plot of axes II and III of both F_A and $F_{\bar{A}}$.

There is a group of extended vectors plots not customarily carried out —those involving the first factor axis on which all vectors are made to have unit projections by the extension process (cf. [21], pp. 225–228, and [15]). Their value is that they provide side views of the positive manifold. What is important here is best seen by considering whether it is possible for vectors near the corners of the configuration *not* to lie at the outer edges of the fans of vectors that appear on these plots. The answer is that, if there is a full set of good corner vectors, at least one must lie at each edge of every fan. Thus the three vectors chosen earlier all lie at the outer edges of the two fans of vectors that appear on the I–II and I–III plots of F_A and $F_{\bar{A}}$ (even the unextended plots show this), and might well have been chosen as corner vectors by inspection of these plots only. Perhaps these same plots may prove useful in carrying out the direct rotations that are usual in factor analysis, e.g. those described by Harris [15].

There is no reason why all possible methods for isolating a set of corner vectors cannot be combined for increased power. Sometimes the centroid of two or more item vectors would make a better corner vector than any single one. Such centroids could be averages, for each axis of F_A and $F_{\bar{A}}$, of raw, normalized, or extended loadings. However, it will not always be possible to locate a full set of good corner vectors; and in that case other rotational approaches now to be discussed may be required.

VIII. Discussion

Once the latent structure rotational criteria are made identical with factor analytic criteria, much additional rotational methodology becomes immediately applicable. For example, if high-speed computing facilities were available, some of the analytic rotational procedures (cf. [3], [4], [16], [19], [20]) could be tried on F_A and $F_{\bar{A}}$ combined, possibly with some additional graphic adjustments. Dr. Kaiser and Dr. Wrigley have suggested in conversation that, since a strictly positive manifold is sought, a transformation that maximizes the sum of the third (or other odd) powers of the resulting projections might serve the purpose.

Possibly the most effective way in which the geometric considerations of this paper can be used is in conjunction with analytic latent class solutions already existing ([1], [7], [12], [18]). If, in a particular empirical application, one of these solutions should yield some latent marginals that are negative or above unity, the whole obtained set of latent parameters could be converted into an F_A and an $F_{\bar{A}}$ and some graphic adjustments applied to eliminate the violations. This would have the interesting effect of leaving the fit of the joint proportions matrix P invariant while altering the fit of higher-order joint frequency matrices. This is not out of keeping with a growing number of suggestions (cf. [9], p. 142) that minor changes in higher-order data may have drastic rotational effects, so that, conversely, sizeable rotations may have only minor effects on the reproduction of higher-order data. More research is needed on this point. If it is borne out, there might arise, in view of the sampling instability of higher-order joint frequencies, some statistical justification for seeking a centred solution regardless of what is indicated by higher-order data.

IX. Conclusion

As an aid in achieving a general discrete class latent structure solution all of whose latent parameters lie within the unit interval, this paper has converted the rotational criteria of an earlier geometric solution into those for a best-fitting orthogonal positive manifold. It is hoped that these criteria will prove useful not only in cases requiring a rotational solution where higher-order manifest data are not called for, but also cases involving analytic solutions where the use of higher-order data has produced inadmissible latent parameters.

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NOTES AND CORRESPONDENCE

ROTATION AND PSYCHOLOGICAL SPACE

A Reply

By R. F. Jarrett

Dr. Burroughs understands most of my argument. He summarizes it succinctly (though not quite precisely) when he says that I think the problems I was discussing arise because psychologists generally seem not to recognize that " standard scoring changes the scale without materially changing the unit ". (His only inaccuracy is in the use of the word ' unit ' instead of the word ' dimensionality '.)

He points out that there are such things as dimensionless variables. He notes that all standard scores are ratios of variable quantities to other quantities expressed in the same arbitrary units. And he believes that all standard scores are therefore pure numbers—dimensionless. He would be quite correct were he to infer that I do not believe this.

I agree that there are dimensionless variables. They include variables which are themselves ratios of *variables* whose values are expressed in identical units. I steal from Norman Campbell (*Physics, the Elements*, p. 399) the example of the eccentricity of conics—the ratio of two variable lengths (variable across conics). But the ratio of a *variable* to a *constant*—even though that constant may appear to be expressed in the same units as the variable in the numerator—is *not* a dimensionless variable but has the dimensionality of the original variable.

Standard scores are variables of this latter kind—although the numerator is a variable, the denominator is a *constant*—and contrary to appearances, a constant whose units are *not* identical with those of the numerator. What appears to be division by the standard deviation merely changes the size of the units employed in describing the object in question. This procedure does not in any way alter the *dimensionality* of the variable with which we must deal; it merely *expresses* that magnitude in terms of another (idealized) unit concerning which we know no more than we know about the original unit.

This is easy to see in the situation in which we restrict our attention to physical variables. When I divide height in inches by 1/2·54 inches = 0·394 inches, I do *not* by this operation suddenly generate a new variable which is not a length. In point of fact, this operation has not been described completely in the above sentence. The incompleteness is in the failure to make explicit the *unit of length* to which this operation converts the original measurement. Properly I should have noted that the transformed variable was to be obtained from the original by division, not by 0·394 inches, but by 0·394 in/1·000 cm, to adopt a common convention. Had the transformation been so expressed, it would have been obvious from the beginning that it leaves us with a magnitude of the original dimensionality.

The standard score transformation is neither more nor less than this. It differs from it only in the source of the constant of the transformation. In my example the constant arises from a comparison of the standard yard with the standard meter. In the case of the ' standard score ' the constant arises simply as a property of a set (usually arbitrary, but often thought of as representative in some sense) of such objects. It remains a constant and not a variable, and it is of the form:

$$\frac{\sigma \text{ original units}}{1{\cdot}00 \text{ ` standard units '}}.$$

Suppose I transform a set of heights of British men into standard scores by subtracting 67·46 in. from each measured height and dividing the resulting differences by 2·57. (The data are of 1883 vintage, quoted by M. G. Kendall, *Advanced Theory of Statistics*, I, p. 8.) Surely no one can argue that the resulting quantities are not *heights*! To be sure, these heights are now measured, as it were, from the top of a table 67·46 in. high rather than from the floor, and they are expressed in terms of glugs—a unit which I have just named and which is defined by the relation: 1·00 glug = 2·57 in. But the resulting numbers are still concrete numbers, not pure numbers, and the variable

$$X' = \frac{(X-67\cdot46)\text{ inches}}{\dfrac{2\cdot57\text{ inches}}{1\cdot00\text{ glug}}} \quad \text{remains a length.}$$

I have been seeking the origin of the strange notion that standardization reduces all scores to pure numbers; but I have been unable to find it. Certainly the early writers (*e.g.*, Galton, Pearson, Thorndike) never made the assertion.

It would seem that two factors may be responsible for the view that standard scores as variables are dimensionless. One of these is the possibility (realized in standard scoring, as Galton first noted) of finding a sense in which a meaningful answer may be given to the question whether an individual is more extreme with respect to one variable than with respect to another incommensurate with it. The second factor is the rather remarkable way in which the very different measurements obtained by two observers with the same set of objects but different units of measurement (and even different absolute magnitudes as origins) are brought into perfect agreement by standardization. These two circumstances apparently led to the conclusion that the meaning of a standard score "does not depend upon the unit", and then to the generalization—quite unwarranted—that standard scores are the pure number values of dimensionless variables.

Let me give an example of a situation in which Dr. Burroughs' argument is invalid. Suppose in a study of physical objects we should measure volumes and masses of a set of objects. Suppose further we should just have *happened* to elect to use a unit of volume in terms of which the standard deviation of the volumes of our objects is exactly 1·000 (of our original units of volume), and suppose we just *happened* to select a unit of mass in terms of which the standard deviation of the masses of our objects is also exactly 1·000 (of our original units of mass). We now convert the original measurements into standard scores and use them rather than the original scores for analysis. If Dr. Burroughs' argument were correct, we should now no longer need to worry about the fact that mass is not the same property as volume—we should have two dimensionless variables. Dr. Burroughs' argument implies, for example, that we might now, in confidence of the meaningfulness of the resulting composite variable, add together the 'standard score' in volume and the 'standard score' in mass. Had we done this with our original variables (even after converting to deviation scores) we should have been reproached for trying to add units of volume to units of mass. But even after we have converted our deviations to standard scores can we reasonably claim that our figures are now 'pure' or dimensionless numbers? I think not. We are still dealing with the *dimensions* of volume and of mass, and a variable which is a linear composite of volume and mass is physically meaningless.

So it is with all 'standard scores'. If there is a dimensionality problem before standardization—as there is when we deal with psychological variables in factor analysis—the problem is unmitigated by the standardization operations. We have no choice but to acknowledge that whether we *express* our magnitude in raw score units (which in psychology are frequently very imperfectly understood) or as standard scores, we are speaking of the *same* magnitude; only our mode of expression has changed—the unit is different, but the dimensionality is the same.

Dr. Burroughs errs, too, in his assertion that the correlation coefficient, *because it is a product of ' dimensionless numbers '* is itself dimensionless. His error lies, not in his assertion that the correlation coefficient is a dimensionless variable, but in the reason he gives for its dimensionless character.

We have just seen that standard scores are not dimensionless, only measured in conventional ' standard ' units, one of which is equal to the standard deviation of the distribution of transformed scores. Consequently, the mean product of standard scores cannot be dimensionless either. What has gone wrong? We are the victims of an algebraic convention, a bad pedagogic practice, and a verbal habit. The algebraic convention is that of suppressing factors of unit from algebraic expressions; the bad pedagogic practice is the unfortunate but common one in teaching elementary statistics of presenting even real data as though only ' pure numbers ' were the objects of our interest—of suppressing the *units* in our algebraic expressions; the verbal habit is simply that of verbalizing (' mean product of standard scores ') the resulting horribly mutilated expression for the correlation coefficient.

Properly speaking the correlation coefficient is a function—of a particular form—of the observations in a bivariate frequency distribution. Its form originally derived from the role it plays in the description of bivariate *normal* distributions, and that form *defines* the correlation coefficient. Perhaps it is most simply expressed in the deviation score form:

$$\frac{\dfrac{\Sigma(X-X)(Y-Y)\,[x\text{-units}]\,[y\text{-units}]}{N}}{\sigma_x[x\text{-units}]\ \sigma_y[y\text{-units}]}$$

and it is in this form that we must examine it.

When the two variables involved have been expressed in standard score form the denominator contains two factors of unity—the standard deviations of the two standardized variables. But our algebraic convention encourages us to suppress them and then to read ' mean product of standard scores '. And now we are undone!

I am, I fear, unregenerate. I rather hope I may have corrupted a few others—though the problems which my analysis of this situation raises are far more disturbing to me than those they solve.

THE PSYCHOLOGY OF VALUE

To the Editor, British Journal of Statistical Psychology

Sir,—I feel that Professor Burt's brief description of my position in regard to the status of value (this *Journal*, XIV, p. 164, footnote 2) is not correctly inferred from the content of our previous correspondence; and I should therefore like to clarify what was really implicit in my letter on the general aesthetic factor. I take it we have now moved to the wider underlying issue of the status of value in psychology.

It is correct that I hold value not to be objective. It is not correct that I think value has no place in psychology. But I certainly believe psychology to be a natural science. The main object of psychology, as I understand it, is to construct internally consistent and operationally testable accounts and predictions of human behaviour. Concepts may be introduced at different levels of description or with different degrees of generality in scientific explanation; accounts of the place of value as a concept in psychology are exposed to difficulties in this respect, because psychology includes in its subject-matter beliefs and acts of choice, which the persons observed to be valuing verbally justify by using language in such a way that the syntax suggests that value is objective.

In order to state my position more precisely, I will try to distinguish by example levels of description in the data and in the theories of psychology, and then indicate where I think we might treat of value as though it were objective, and where we should not do so.

(i) A subject reports a hedonic tone on introspection, perhaps as a pervading quality of his experience. This report in itself is a datum for the psychologist and a verbalization of experience for the subject. Both are fit material for study; and it would be wrong to exclude them from psychology, because we want to classify and predict the occurrence of experiences and the ways in which we verbalize experience, whenever we do verbalize it.

(ii) A subject by making statements attributes value to a thing which he considers on non-evaluative grounds to be external to himself. Such statements appear to be predicating value as an objective quality of the thing in question. I think that within psychology we ought instead to regard them as evidence of a relation between the subject and the thing. This relation is valuing; and a property of relations is that they only exist between terms, not in the absence of one or other of the terms, i.e. the antecedent or the consequent as they are sometimes called (Bochenski 1959).

(*a*) 'I like this shade of red' is a statement which the psychologist can treat as a raw datum, if he has additional information about the 'I', the 'like' and the 'shade of red' referred to in the statement. This additional information may be needed to establish whether the statement is peculiar to any one subject, to a group of subjects, to a number of shades physically or subjectively defined, and to a number of viewing conditions physically or subjectively defined; the subjective definitions possibly include value statements by the same subject whose original statement we are taking as a datum. Statements such as 'I like this shade of red' are incontrovertible at their own level, because they are about a personal experience of the speaker, and that experience is indisputable. But it is the report of the experience which is incontrovertible, not the reported object of the experience: otherwise we could have no operational definition of illusions.

(*b*) 'This is a nice shade of red' is, if we are using language or analysing language in the usual way, a statement about a shade of red. If we adopt the extreme positivist position, it is a meaningless emotive noise, not analysable in the way some other sorts of language are analysable by philosophers. Psychologists would make things unnecessarily difficult for themselves as scientists if they treated all statements ostensibly predicating value as meaningless, because then all such statements would be members of a single null-class in terms of meaning. We may get round this problem by saying that 'I like this shade of red' is behaviourally equivalent to 'This is a nice shade of red'; both indicate that valuing is taking place. Obviously statements of the forms (*a*) and (*b*) are not grammatically the same; I am making them the same for the purpose of constructing predictions of behaviour without positing value as objective. Statements such as 'This is a nice shade of red, but I do not like it' are treated in this context as equivalent to 'S values R in one sense and does not value R in another'. We are not obliged to have only one sort of valuing, though we are obliged to minimize the number of sorts of valuing we use to account for behaviour. A factor analytic approach may be used to do the minimizing.

(*c*) 'This is a nice shade of red' may be said by a psychologist who is telling a paint-maker about the results of an experiment on preferences for paint-colours. At this level of description we can say 'The red is nice' because we mean, as a probability statement, "Presenting this shade of red (physically defined) to a number of human subjects (physiologically defined as normal in sensory capacity or psychologically defined in terms of a common response or pattern of responses in a previous situation) leads to various statements which are logically or syntactically equivalent to 'I like this shade of red', or leads them to choose this red when it is presented along with other reds."

At this level of description 'nice shade of red' has an operational definition, because it is a terse way of referring to a consensus of choices at a point in time. It is one way of referring to a shade of red which has been the 'consequent' of a number of valuing relations. We may go further, and represent the set of choices as a set of points on a unidimensional continuum. In mathematical language we may perhaps further express it by saying 'Let there exist $u(x)$ upon x, where u is a value function and x is a stimulus magnitude '. I take this to imply that value has a place in psychology as a concept at this level of description.

All the above seems to me to be acceptable without any idea of value as objective. The assertion that value is objective, which is usually found wedded to the notion that value is absolute and not relative, is to my mind logically irrelevant to the statistical psychologist. I have argued this at length elsewhere (Gregson 1961); and the above is merely a précis of my views.

I am in complete agreement with Professor Burt when he says that ' psychology . . . has evidence to offer on what were traditionally treated as problems of epistemology ' (this *Journal*, XIV, p. 77). I might be tempted to go further, and say that psychology has evidence to offer on what are currently regarded as problems of linguistic analysis. My disagreement with Professor Burt seems to centre on what are the philosophical problems to which psychology might contribute evidence. A psychophysical experiment to find the subjective weight of a unicorn's horn would not be difficult to devise, but this does not mean we should try to contribute to the nominalist-realist controversy.

R. A. M. GREGSON

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THE SENSE DATUM THEORY. I.

To the Editor, British Journal of Statistical Psychology

SIR,—Now that the behaviourists have begun to relax the more rigid prohibitions originally laid down by their leader forbidding all mention of conscious experience, the problem of perception has come once again to the fore. In the meantime, however, the significance of the term has apparently changed, and the word itself has become increasingly ambiguous. In a recent book, edited by Professor Humphrey and Dr. Argyle, the word is used, we are told, to denote ' a man's general orientation to his environment '; and the author of the experimental chapter on the subject actually prides himself on not defining the word at all (*Social Psychology through Experiment*, 1962, pp. 20 and 32). Statistical psychologists, like Thurstone and Roff, claim to have found as many as eighteen different factors entering into what is called ' perception '. Professor Burt, on the other hand, apparently found only two (' The Structure of the Mind: A Review of the Results of Factor Analysis ', *Brit. J. educ. Psychol.*, xix, pp. 109f.). Thus with contemporary psychologists ' perception ' seems to have acquired a connotation very different from that which it possessed in pre-behaviouristic literature. It is therefore doubly welcome to find Professor Burt now ready to explain his own views on the issues thus raised. Nevertheless, in my humble opinion his recent pronouncements tend to complicate the whole issue quite unnecessarily, and to increase rather than diminish the confusion and the ambiguity.

In the last issue of this *Journal* (XIV, p. 176) he tells us that, when " the unprejudiced psychologist opens his eyes and describes what he sees," the true view of his experience involves " an immediate and intuitive apprehension of concrete material objects seen out in space almost from the very start . . . The colour, shape, and solidity are apprehended as essential characteristics of the thing itself." In other words, Professor Burt comes down on the side of the most naïve type of so-called ' naïve realism '. He himself describes it as the view of ' common sense ', though one could wish that he would explain a little more clearly what he intends by that rather loose designation. Surely the views of common sense are merely the crude and primitive ideas developed by our prehistoric ancestors, and handed down with other semi-superstitious notions by popular speech and antique tradition.

And on this basis he apparently maintains, in opposition to Dr. Gregson's criticism (this *Journal*, XIV, p. 73), that " the beauty of nature is as objective as its sounds and colours." It is true his actual wording is: " *if* we could accept such an account [*sc.* that of common sense] at its face values," then the foregoing corollary would follow; but that, I take it, is merely a rhetorical concession.

As a psychologist, however, Professor Burt must be fully aware that our 'intuitive perceptions' (as he calls them), so far from being 'objective', are frequently found to be false. One of my own children has just offered me a realistic apple made of wax; I was completely hoaxed. Every morning I see a non-existent face and hand in my mirror as I shave. Or, to take a more extreme case, the drunkard in his delirium sees all sorts of non-existent objects—pink rats and coiling snakes. What then are the perceptible differences between the pink rats seen by the drunkard and the grey rats I myself have seen by the water's edge? Between the non-veridical rat and the veridical rat, between the non-existent hand in the mirror and the real hand that wields the razor, there are for the percipient no intrinsic differences whatsoever. Yet we all agree that the pink rat is just a hallucinatory image, like the pink after-image that I see after looking steadily for sixty seconds at a bright green patch of light. Both are sensa in the same sense. Both therefore must be subject to the same rules of interpretation. Consequently, if our senses deceive us in the one case, they may deceive us in all. And if in the end I succeed in persuading myself that grey rats really do exist, it will not be simply because I happen to have seen them.

What I really see in all such cases is (as Professor Broad has so convincingly argued) just a *sensum*. The 'object' is no more than a plausible hypothesis. Oddly enough, when he is discussing problems of *pure* psychology—e.g. in his former controversies with the behaviourists—Professor Burt himself has put forward a very persuasive version of the so-called sense-datum theory, insisting (rightly as I should hold) on the unique nature of the cognitive relation between the 'subject' and the 'sensum' which the subject 'immediately apprehends'. Why, then, when he turns to discuss the problems of *applied* psychology (e.g. those of aesthetic and ethical psychology) and the deeper issues of philosophy, does he apparently discard this extremely simple and satisfactory account of the processes of perception? Once we accept the sense-datum theory the multiplicity of factors discovered by statistical psychologists becomes intelligible, for perceptual 'data', unlike the perceptual process, are undoubtedly multiple.

Having once recognized how deceptive sense experience can be in these more exceptional examples, it becomes easy to recognize its deceptiveness in our ordinary daily life. When I look at a penny, as I twist it about in my hand, what I actually see is a series of brown elliptical shapes, varying greatly in width, and diminishing in size as I hold it at a distance; and yet the 'real penny', if there is such a thing, is circular. The varying shapes that I actually see are not only inconsistent with the presumable shape of the real object, and with the shape I feel with my fingers; they are inconsistent with each other. But if, as Professor Burt maintains, what I 'immediately apprehend' were the real penny, then it should, in the interests of consistency, have a constant size and a constant shape. Surely these commonplace distortions are themselves sufficient to refute the 'common sense' theory, and to demonstrate that the socalled penny is simply a mental construct.

When I look out on the world, the scene that I see is virtually a photographic image of the scene as it would appear if photographed by a camera with its lens situated in the same position as the lens in my eye. The laws of perspective which govern the distortions of the visual image are identical with the laws of perspective that determine the distortions of the picture taken by such a camera. We consequently have to correct this two-dimensional picture for the perspectival distortions in accordance with those laws (which we implicitly learn); and the world of common sense is the final result. Common sense, however, would have us take all that we thus eventually perceive *at its face value*, as though it were first hand observation rather than a kind of hypothetical and unconscious inference; and so common sense maintains—or at any rate assumes—that the world of colour and sound is wholly objective. But the cumulative effect of the various anomalies I have described (they are of course hackneyed topics in all discussions of the problem) is that what we perceive is essentially subjective.

In his eloquent plea for a revival of the introspective study of conscious phenomena (*Brit. J. Psychol.*, L III, pp. 229f.), Professor Burt appeals for renewed investigations not only of the '*processes* of consciousness', but also of the '*contents* of consciousness'; and among these he expressly includes our sensory experiences. If, however, sensory experiences are (as he elsewhere phrases it) 'contents of the mind', then what is contained *in* the mind cannot possibly be 'external'. But would it not be far simpler to accept the older interpretation, and regard what I have termed 'sensa' as incidental by-products ('unique' if you like) which are generated by the working of the brain—mere emanations of the cortex? Their neural processes are presumably in isomorphic correspondence with the retinal processes, so that what we see is virtually the cinematographic image photographed, as it were, on the retinae. Occasionally we may see equally vivid images (as in dreams or in the hallucinations of delirium) which are not related to any retinal image; and the obvious conclusion therefore is that the necessary and sufficient conditions for the occurrence of a sensum—and for its various qualities and characteristics—are to be found, not in the physical objects which we are alleged to 'apprehend', but in the brain itself. Furthermore, if we accept this conclusion so far as psychological phenomena are concerned, then we are perfectly free to accept the physicist's conclusions in regard to material phenomena—namely, that material objects are essentially aggregates or systems of atoms (which are themselves aggregates or systems of more elementary particles) possessing only such properties as the physicist studies, devoid therefore of all secondary qualities, and identifiable by their location in a four-dimensional universe of space-time. This (as Dr. Taylor has argued both in this *Journal* and elsewhere) is the ideal scheme towards which the psychologist should strive and the conceptual pattern with which psychological thinking should endeavour to conform. In modern science there is in principle only one objective *quality*, and that, paradoxically enough, is *quantitative*.

Quantity alone is 'public', or as I should prefer to say, communicable. All 'qualities' as such are private, incommunicable and derivative. And every philosopher and psychologist who has discussed the subject has acknowledged that sensa—the supreme instances of would-be qualitative attributes—are *necessarily* private. Now the grand advantage of taking a quantitative type of science as our ideal and goal is that, by adopting the axiomatic method as Euclid and Newton tried to do, and aiming at a purely mathematical treatment, we can eventually achieve a single systematic formulation that is at once consistent and complete. That I take it, is the burden of Dr. Taylor's arguments when he urges the psychologist to adopt, not of course Newton's obsolescent cosmological theories, but Newton's logical technique and mathematical methods; and that is surely the ideal of psychology as a quantitative science. After all, is not this the aim of the statistical psychologist, and the reason why we are discussing the problem of consciousness in a journal which is essentially mathematical? If with Professor Burt we endeavour to treat the miscellaneous assortment of sensory qualities—colour, sound, hardness, and the like—as equally objective, then our account can never hope to be either complete or self-consistent. Indeed are not its inconsistencies and its lamentable lack of completeness the crowning scandal of psychology at the present day, and are not these the inevitable consequences of trying to maintain the outmoded interactionist theory?

We may agree that this quantitative conception of objective reality is no more than a hypothesis; but so is the common sense notion of a world of ordinary physical 'things'. And of the two the former hypothesis is assuredly by far the best established. If we are asked how have we managed to acquire the notion of a purely independent material reality when our entire experience is, from the very start, confined to a mere changing pattern of sense-data, then, I would suggest, we can invoke the principles on which Professor Burt in other contexts has laid so much stress—namely, our capacity for 'extrapolating' to entirely novel concepts by the process of 'educing correlates'. Some of our sense-data are more permanent than others; they are independent of the others; they form the regular antecedents of those others; and consequently we treat them as the cause of those others. The small child watches the insubstantial flames in the open grate. He sees that they arise when a lighted match is applied to the more substantial paper, wood, and coal. For him these are originally mere sensa; even the 'substantiality' is no more than a sensum of solidity and weight. The flames are transitory sensa; the match, the wood and coal are *relatively* permanent, and can therefore be regarded as the 'causes' (by which term we must understand simply the prognostic antecedents). When he realizes that the match and the wood and coal, as he sees them, are also just more or less transitory sensa, then by an automatic extrapolation he *educes a further correlate* which is still more permanent and still more independent of his sensory experiences: underlying the glimpse of a bit of coal (he argues) is an objective bit of coal—a 'substance' or 'thing'. And so we all of us come at a fairly early age to invent the ideas of thinghood by an imaginative application of the principle of analogy. Thus, as after all seems obvious, the sensing of the sense-data comes first; the socalled percept or 'object' is a subsequent quasi-logical synthesis. And with increasing knowledge we modify our earlier hypothetical conceptions of these objects to fit in with our general experience; and so in the end, if we are sufficiently pertinacious, we come at last to the newest hypotheses of physical science.

This, I suggest, is how we really get what meagre notions we possess of a physical world outside us—not by any 'immediate apprehension' or a 'direct perception' of things as they actually are. Plainly the physical object *itself* cannot be part of the 'content of the mind'; how then can our sensory experience, which *is* part of the 'content of the mind', possibly give us a 'direct acquaintance' with the existence and nature of those objects? As Dr. Gregson has pointed out (this *Journal*, XIV, p. 73) "a kind of metaphysical absolutism seems to run through Professor Burt's theorizing about both the supposed natural characteristics and the supposed aesthetic characteristics of physical objects." Unfortunately Professor Burt's assumptions are, as Dr. Gregson complains, implicit rather than explicit. May we ask therefore what precisely are Professor Burt's views about the metaphysical status which apparently he would have us assign (*a*) to material objects as he conceives them, and (*b*) to sensory experience or to sensa, if, as he seems to hold, these latter are *both* contents of the mind *and* attributes or properties of material objects?

R. J. ATKINS

THE SENSE DATUM THEORY. II.

To the Editor, British Journal of Statistical Psychology

SIR,—"Is Saul also among the prophets?" To find the chief exponent of quantitative techniques openly proclaiming the objectivity of sensory qualities and aesthetic values will come as a gratifying surprise to every artist, art critic, and to all students who are interested in the psychology and philosophy of aesthetics. The biological psychologist has assured us that art and artistic productions are merely the somewhat sophisticated forms of play that we might expect from an intellectual mammal, when he is no longer engrossed in the more serious business of keeping himself alive or prosecuting scientific research or carrying out technological applications. The physiological psychologist declares that all modes of experience, aesthetic and non-aesthetic alike, are simply by-products of the neural activities of a material brain. For the psychoanalyst poetry and painting are no more than sublimated expressions of unconscious emotional complexes; while the behaviourist dismisses all manifestations of consciousness as beneath scientific notice. However much they differ in their ostensible reasons, all seem agreed that colour and music, and still more the beauty that the artist feels or strives to create, are wholly subjective. Only one contemporary psychologist—Professor Valentine—takes such things seriously; and he too regards them primarily as a subject for laboratory experiment and quantitative analysis.

Unfortunately Professor Burt's pronouncements on the nature of the sensory and perceptual modes of cognition consist merely of a few incidental asides uttered in the course of a book review (this *Journal*, XIV, pp. 176f); and possibly in a journal of statistical psychology it would be unfair to ask for more. Nevertheless, Dr. Gregson has already challenged his "objective or absolutist interpretation", and complains that "Professor Burt has not indicated how a statistical demonstration could be a part of an *experimentum crucis* between an absolutist and a subjectivist aesthetic theory." Personally I should be tempted to reply that the demand that concepts and theories should be rejected as meaningless unless they can be directly checked against "operational definitions" and statistical or experimental "demonstration" begs the whole question in favour of a subjectivist theory. It is like arguing that the very claim to knowledge should be demonstrated by an *experimentum crucis*. Dr. Gregson's main question seems by its very wording to assume the point at issue. "In what sense", he asks, "can the existence of consistent modes of evaluation . . . be relevant to demonstrating that aesthetic appreciation is the perception and valuing of *sense data* in relational form?" (this *Journal*, XIV, p. 72, my italics).

Dr. Gregson, if I interpret him correctly, supposes that all psychologists who consider such matters would, as a matter of course, accept some form of the orthodox sense datum theory. But the theory has many forms; and he omits to say which form he himself endorses, or how he defines the key-phrase. Dr. Hirst in his book has given an admirable outline of the most familiar versions—those of Professor Broad and Professor Price—and offers analytic summaries of their main arguments, couched almost in syllogistic form, and eventually rejects them on philosophical grounds. This seems in keeping with Professor Burt's conclusions, though Burt's grounds are psychological. Professor Burt, however—rightly if I may say so—rejects the alternative theory which Dr. Hirst himself puts forward. But one could wish that he would elaborate his own in greater detail, and show how it would meet all the familiar problems raised by hallucinations, illusions, individual variations in normal perception, and the like.

No doubt much that is apt to bewilder the plain man, when he hears—often at second or third hand—the various criticisms advanced by psychologists and philosophers against the assumptions that seem to him just sheer common sense, is really the result of the past history of certain academic controversies with which of course he is unfamiliar. Yet occasionally even the plain man, when he turns into an artist or art critic (and most artists and art critics are plain men), encounters psychological problems that bewilder him. An Impressionist's view and a Pre-Raphaelite's view of precisely the same scene differ so widely that we often wonder whether one or both of them may not need attention from an oculist. How, for example, does the psychologist account for blurred and indistinct sensory percepts that most of us experience as our sight gets altered with advancing age? Or for the spotty expanses painted by the pointillist and the smooth tone it presents at a distance? At the other end of the field my Plymouth Rock and Sussex hens look grey: should I draw them barred and speckled, as I know them to be, and as I suspect Picasso would? And is the representational painter to reproduce a brick wall as he sees it or so as to show it construction? In short, is he to paint sense-data or his sophisticated perceptions? And should the art-critic, who is bound to talk psychology even if he improvises it, first make a study of the psychology of colour-vision and of perception, and if so should he borrow the jargon of the objective or the subjective school uncritically or may he venture to contribute something of his own? Perhaps either Professor Burt or Dr. Gregson will enlighten us.

W. F. HUTTON.

THE SENSE DATUM THEORY

A Reply

By Cyril Burt

To reply to all the questions raised by those who have contributed to the recent controversies about the ‘nature of the sensory and perceptual modes of cognition’ (to borrow Mr. Hutton’s phrase) would scarcely be practicable here. I shall accordingly confine myself mainly to answering criticisms or inquiries relating to what I myself have briefly stated in my earlier papers. But first of all let me say how fully I agree with Dr. Atkins, when he deplores the loose way in which terms like sensation and perception are so often used by contemporary psychologists, and insists that both experimental and statistical investigators dealing with such problems should start their researches by considering how they intend to interpret such terms.[1]

Most of the present discussion (pp. 165–169 above) turns on the validity of the socalled ‘sense datum theory’. One contributor apparently holds that it alone provides an adequate explanation for the multiplicity of cognitive factors which the statistical psychologists like Thurstone and Roff claim to have discovered in their attempts to analyse the process of perception. What is still more surprising, nearly all who discuss the theory appear to regard it as a comparatively recent innovation. A word or two therefore seems needed at the outset in regard to its history.

The doctrine goes back to the father of British empiricism, John Locke. It is in fact merely one of many possible forms of the so-called ‘representative theory’ of perception. What today is commonly termed a ‘mental content’ Locke calls an ‘idea’. This he defines as “the object of the mind when it thinks”. Ideas may be either simple or complex. ‘Simple ideas’, which alone concern us here, are those which reach us through an external or an internal sense. Later British psychologists substituted the term ‘sensation’—an equally ambiguous word whose employment led to much confusion: like so many nouns with the same termination, it ought to mean an abstract process, namely, the process of sensing, but it is also used to designate the concrete object of the process, i.e. that which is sensed. German writers adopted the term ‘Vorstellung’, which earlier translators rendered by the word ‘idea’ and later writers—like Ward and Stout—by the more accurate term ‘presentation’. And the founders of modern scientific psychology, notably Wundt in Germany and Titchener in America, made the experimental study of sensations the basis of their entire theory of the mind.

The central doctrine of the sensationist or atomistic school, as it came to be called, was that all mental processes result from the compounding of sensations (including under this term, not only those sensations which are ‘peripherally excited’, but also those replicas of them which are ‘centrally excited’, i.e. mental images)—very much as molecules were said to be compounded of atoms. Towards the close of the nineteenth century, however, the whole notion was subjected to drastic criticism at the hands of the later introspectionists.

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[1] In the statistical investigations to which Dr. Atkins refers, I myself used the term ‘perception’ in the narrower sense adopted by earlier writers, as indeed was stated in the reference that I quoted: this, I think, explains why so few specific factors then emerged. The later writers whom he cites use the term as a synonym for what my collaborators and I have called ‘observation’. In the processes falling under this broader head we discovered a far larger number of independent group factors (synthetic apprehension, speed of recognition, relevant vocabulary, visual imagery, spatial ability, and sensori-motor ability). I may perhaps add that the factors found by Thurstone and Roff in the researches Dr. Atkins quotes, were *correlated* ‘primary abilities’. Quite recently, however, Dr. Mukherjee (of the University of North Carolina) has re-factorized Roff’s eighteen ‘first order factors’, and found that most of the factor variance of his seventy tests could be virtually accounted for by only seven ‘second order factors’ (‘Second Order Factor Analysis of the AAF Perceptual Tests’, *Perceptual and Motor Skills*, XV, 1962, pp. 283f.).

" A pure sensation ", said James, " is a sheer abstraction ": from the outset what the infant perceives " has unity, objectivity, and substantiality, in the full sense which any object of adult perception has these things ". But, " as we can only think about objects with which we already have *acquaintance*, we postulate a function whereby we first become aware of the bare *immediate natures* by which our objects are severally distinguished ".[1]

The Relational School. Meanwhile, the Austrian school, led by Brentano, had begun to recast the traditional doctrine into a more satisfactory shape. Brentano revived the scholastic conception of ' intentional inexistence ', and endeavoured to classify all mental activities according to the way in which the ' subject ' ' intended ' (i.e., referred to) an ' object ' of consciousness. In the most developed form of the theory, that of Twardowski[2], a sharp distinction is drawn between (i) the mental *Akt* (i.e., the mental process or relation—in cognition, for example, a process of direct apprehension), (ii) the mental *Inhalt* (or content), and (iii) the *Gegenstand* or object. It was, however, Bertrand Russell who gave the doctrine its clearest and most persuasive formulation. " Acquaintance ", he says, " consists essentially in a relation between the mind and something other than the mind; . . . the faculty of being *acquainted* with things other than itself is the main characteristic of a mind." And it is to Russell that we owe the term ' sense-datum '.[3] "Let us ", he says, " give the name ' sense-data ' to the things immediately known in sensation—such things as colours, sounds, smells, hardness, and so on. The colour is that of which we are *immediately aware*—that with which we are directly acquainted. The awareness itself is the ' sensation '. The real table, if it exists, we will call a physical ' object '." Thus the word ' sensation ' is now restricted to its proper meaning and denotes the process; and the phrase ' sense datum ' is introduced to denote what was formerly called a ' simple idea '. Two modes of cognition are recognized: (i) the direct cognition of sense data, which Russell calls " knowledge by acquaintance "; and (ii) the indirect cognition of physical objects or things, which he calls " knowledge by description ", though that, we are told, really " rests upon acquaintance as its foundation ".[4]

This view, it will be observed, requires us to recognize *two* epistemological objects. When we perceive a coloured thing, the red sun as it is setting, for example—the red that we ' sense ' is not identical with the sun, though both it and the sun are objects of our cognition or thought. The colour may be caused by the sun, or by energy coming from the sun ; it may possibly represent or imply the sun; but in itself it is (to borrow the Austrian terminology) a mental 'content'. Since it is directly known, it is something 'given'—in short, a 'datum'; and, since it is known through the senses, it is a 'sense-datum'. Incidentally we may remark that, in thus assuming two distinct epistemological objects—the one physical and the other mental—we are necessarily committing ourselves to an epistemological dualism. If we regard the 'subject' of the cognitive relation as likewise in some way mental—a mind rather than a brain—then we are also committed to a psychophysical dualism.[1]

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[1] W. James, *Principles of Psychology*, 1890, II, pp. 3, 8f. (his italics). Cf. J. Ward, *Encyclopaedia Britannica*, s.v. ' Psychology '. Ward's contention was that a sensation was an abstraction not so much from distinguishable objects, but rather from the *totum objectivum*, i.e. the ' presentational continuum ' or ' field of consciousness '.

[2] K. Twardowski, *Zur Lehre vom Inhalt und Gegenstand der Vorstellungen*, 1894; Brentano, who uses both words (*Inhalt* and *Gegenstand*), apparently drew no sharp line between them. In this country Brentano's views first became known through G. F. Stout, who also adopted " the *mode in which consciousness refers to an object* as the basis for classifying ultimate mental functions " (*Analytic Psychology*, 1896, esp. Vol. I, pp. 40f.).

[3] According to Professor Braithwaite (*Proc. Brit. Acad.*, XLVII, 1962, p. 297) Russell took the term from unpublished lectures of G. E. Moore (see Russell's own preface). Moore originally used the phrase ' sense-contents ' (cf. *Philosophical Studies*, 1922, p. 79); but later substituted ' sense-data ' (*Some Main Problems of Philosophy*, 1953, p. 39). However, owing to Moore's reluctance to publish, it is to Russell's exposition that the wide currency of the term is really due.

[4] *Problems of Philosophy*, 1912, pp. 17–18. An alternative definition is that of Broad: " I give the name of ' sensa ' to the objective constituents of perceptual situations on the supposition that they are *not* literally parts of the physical object we are said to be perceiving " (*The Mind and its Place in Nature*, pp. 181–2, his italics). Since the supplementary ' supposition ' begs one of the main questions that I want to discuss, I should prefer an ostensive definition, such as Russell's. As ordinarily defined, sensa would seem to differ from physical objects in being (i) private, (ii) qualitatively heterogeneous, (iii) transient in time, (iv) spatially incomplete, and (v) dynamically devoid of causal properties.

Russell, in common with most writers who have discussed the problem, notes that "the judgements that common sense bases upon perception, such as 'There is a dog', usually go beyond the datum: when you think you see a dog, what is really given should be expressed in the words 'There is a canoid patch of colour'." In his later writings[2] he carries his phenomenalism further still. Instead of the 'inferred objects' of earlier writers we ought, so he contends, to substitute 'constructions' made up of sense-data and sensibilia, and so get rid of one of the two types of object. As a result Russell's dogs and cats become logical rather than zoological entities.

Sense Perception in Ordinary Life. In order to test these various theories let me ask the reader to follow me in a few unbiased attempts at introspection. And let us begin by inquiring how far the sense-datum theory, as Russell expounds it, really fits the facts of ordinary everyday experience. Consider the first conspicuous thing your eyes happen to light on as you sit down at your table. Lying on a tray in front of me, I see a golden sovereign, which, according to a venerable custom of my school, I received as a boy at the Mansion House from the Lord Mayor. It is an ancient souvenir, fondly treasured by my family, which someone has just unearthed from a long-forgotten drawer. A champion of the old sensationist doctrine would describe my present perception by saying that I see an elliptical patch of shining yellow, which, through association, suggests first certain imaginary sensations of touch and weight such as I might feel if I picked the thing up, then a specific name—'sovereign', and finally the intimate history of this particular specimen. These various associations must have been acquired from past experiences when as a youngster I first examined and handled objects of this kind; and from the transient sensations, actual and revived, which I now experience, I infer as a plausible hypothesis (so it would be argued) the existence of a solid circular disc of metal, weighing a little over a quarter of an ounce, reflecting light-rays predominantly of 0·0006 cm. in length or thereabouts, possessing continuous existence even when hidden from human sight, and boasting a peculiar history of its own. This, or something like it, couched in more general terms, was the account of perception given by most of the leading British, German, French, and American writers throughout the latter half of the nineteenth century—Mill, Spencer, Bain, Helmholtz, Wundt, Binet, Titchener and their numerous disciples.[1] The essence of the theory might be summed up in the formula: ' sense-datum + associated memories = percept '. And this attractively simple scheme has appeared and re-appeared in many popular textbooks, particularly those written by physiologists, down to the present day.

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[1] *An Inquiry into Meaning and Truth*, 1940, p. 139.

[2] He still adheres to the definition quoted above, but rejects the rest of the theory. The retraction appears first in his later book entitled *Analysis of Mind* (1921), where the relational view of cognition is now explicitly discarded. As he puts it, "no mental occurrence has that sort of relational character that was implied in the opposition of subject and object, of knower and known. We reach this result by following out to its logical conclusion the behaviourist definition of knowledge. . . . Seeing a leaf consists simply of the existence, in the region occupied by our brain, of a green patch causally connected with a series of events emanating from the place where physics places the leaf" (*An Outline of Philosophy*, 1927, pp. 225, 292). And again, "the dualistic view of perception as a relation of a subject to an object is one which—following William James—empiricists have now for the most part abandoned. The distinction between 'seeing the sun' as a mental event and the immediate object of my seeing is now generally rejected": and with this (so Russell declares) "I concur". According to this interpretation the only 'event' is the transient existence of a sun-like sense-datum. But every sense-datum, we are told, occurs in a double context. When I consider such a datum as part of my own biography, I speak of it as something which 'I' see; when I consider it in another context, that of the observations of other people as well as of myself, then I regard it as part of a 'physical thing'. Yet both the personage that I designate by the first personal pronoun and the physical thing that I designate by the name of 'sun' are merely convenient logical fictions, built up from sense-data (cf. *Human Knowledge*, 1948, pp. 220f.). All this of course is tantamount to a thoroughgoing phenomenalism. A quarter of a century ago it became for a while a highly popular philosophical creed, thanks largely to the brilliant advocacy of Professor A. J. Ayer (*Language, Truth, and Logic*, 1936). Ayer, however, has since abandoned it (cf. *The Problem of Knowledge*, 1956, pp. 138f.); and today it finds few supporters.

Now I contend that this description, even in its more sophisticated forms, in no way corresponds with what we actually experience when in our ordinary life we perceive such things as coins or cups, plates and knives, lying on our tables. When (for example) I noticed that Victorian coin, what I saw was *not* just a coloured patch with no particular location,[2] but a solid metallic object out there in space. The gold sovereign really *looked* solid, much as the red bars of my electric fire *look* hot. It is useless for Bain and his followers to contend that sensations of vision cannot possibly convey impressions of solidity or of warmth. Such an argument may sound logical enough; yet it is flatly contradicted by what we directly observe in our own experience. Nor do the impressions of solidity, distance, and the like, arise as mere supplementary associations, progressively acquired by dint of past experience. The behaviour of tiny infants and of intelligent young animals—

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[1] Once again it goes back to Locke. " Had we senses acute enough " (he writes) " to discern the minute particles of bodies, and the real constitution on which their sensible qualities depend, I doubt not but that they would produce quite different ideas; that which is now the yellow colour of gold would disappear, and instead we should see an admirable texture of parts of a certain size and figure " (*Essay concerning Human Understanding*, Bk. II, ch. 28). The wording in the text (with a few slight modernisations) is that of J. S. Mill, when describing his own perceptions of " a piece of gold " (*Logic*, I, p. 61). Contemporary writers seem to prefer as their favourite example a penny, an orange, or a tomato—always something that can be readily picked out and picked up.

Mill, it may be noted, virtually anticipated Russell's phenomenalist doctrine of ' logical constructions ', since he goes on to maintain that a ' physical body ' is nothing but " a set of sensations, or rather of *possibilities of sensation*, joined according to a fixed law " (*ibid.*, p. 63). Spencer (followed by Sully) speaks of perception as a ' presentative-representative process '; in his terminology the yellow patch is a ' presentation ', and the memory-images of touch, weight, etc., which it calls up by association, are ' re-presented ' sensations. " The sensations of vision, e.g., the feeling of colour ", says Bain, " by itself can convey no knowledge of any outward object; nor do I see how with the eye alone we can ever pass from the internal consciousness to the external perception." The recognition of such qualities as size, form, distance, etc., he explains, result from muscular activities—of the eye as well as the hand—whereby we acquire permanent and revivable impressions. Through past experience " the luminous impression has become associated with definite muscular feelings "; and the " strength of the association " determines " the closeness of the adhesion " linking the sensations obtained through the eye with the revived ' images ' of the muscular and tactile sensations. Helmholtz regards the passage from sensation to physical object as a process of ' unconscious inference '. Later writers prefer to say that the visible shape and colour come to ' symbolize ' the object; they are, in Peirce's oft-quoted phrase, " *signs* developing according to the laws of inference ". Dewey puts it behaviouristically by saying that perception consists in " behaving to the *given* situation as involving something *not given* ": (see for a fuller account of the ' symbolic theory ', C. W. Morris, *Six Theories of Mind*, 3rd imp., 1950, pp. 282f.).

[2] The correction is one of the debts that psychology owes to the early Gestaltists, though in this they were largely anticipated by Ward and Stout. We do not see (as Russell's original statement implies) just a ' patch of colour ', i.e. a simple sensum; we see (in his modified phrase) a ' *canoid* patch ', i.e. a pattern—a meaningful *Gestalt* standing out from a background, as a ' separate thing ' (see Ward, *Psychological Principles*, p. 161, ' The Intuition of Things ', Stout, *Manual*, Bk. III, pt. ii, ch. 2).

chimpanzees, for instance—strongly suggests that they see the things that catch their attention as solid, tangible objects ' out there ', almost from the very start—as soon as the requisite mechanisms mature.[1]

Accordingly, it is tempting to suppose, as the so-called ' naive realist ' is said to do, that in perception we have an immediate cognition of the object ' as it really is in itself ', i.e., as it is independently of any human perception. In other words, that the permanent thing perceived is not a mere collocation of atoms or molecules, or (in Locke's phrase) " a texture of minute particles ", much less a set of mere " possibilities of sensation ", but a concrete and complex existent, which (whatever else it may be) has the *colour* and *shape* which under normal conditions not only I, but most other human beings, would assign to it.

The Logical Basis of the Sense Datum Theory. At this stage in the argument, however, some critic will step in to remind us of the proverbial fallibility of our eyes and ears:

' Sense, Man's first instructor, while it showes
To free him from deceipt, deceives him most '.[2]

And our textbooks are stocked with a baffling series of anomalous phenomena which appear to be wholly incompatible with the simple-minded view set forth above. Most of the phenomena — hallucinations, illusions, and erroneous perceptions of various kinds — may be classed together under the heading of ' delusive perceptual situations ' (a phrase I borrow from Professor Broad). They are admittedly exceptional cases; yet they occur often enough in every day life to confirm the feeling that the ' naive ' judgements which we commonly base on our perceptual experiences make far stronger claims to knowledge than our perceptions by themselves could possibly justify. Remembering how often I have been deluded in the past—seeing mirages in the desert, ghosts in the churchyard, hallucinatory daggers, realistic duplicates in the looking glass, and all the other deceptive apparitions to which philosophers and psychologists seem peculiarly prone—surely I ought first of all, in the spirit of Macbeth on a more startling occasion, to inquire " Is this a coin that I see before me? Come let me clutch thee." If the sense of touch assures me of the solidity and roundness of the sovereign, and if other persons in the room testify that they too see exactly what I see, then, says the critic, I may legitimately infer (provided no conjuror or spiritualistic medium is present) that there *is* a coin, and, since it turns out to be a 'public' object, that it is a suitable thing for the assayer to weigh and measure, and the chemist to analyse. But so long as I confine myself to my own visual experience, all I can reasonably assert is (so I am told) not that 'I perceive a coin' (since that implies that an objective coin is actually there for people to perceive), but merely that 'I seem-to-perceive a coin'. If I accept the critic's argument, then I have already taken the first step towards the sense-datum theory. The second step is to translate this assertion into the rather different statement that "I perceive what-seems-to-be a coin". We then go on to distinguish between the 'seeming-coin' and the real or 'physical' coin. And this leads us to the third step: the assertion "I seem to see . . ." or "I appear to see a coin" is equated with the very different statement, "What I see is the appearance of a coin"; and the 'coin-like appearance' thus postulated is now treated as an independent non-physical entity, existing in its own right.

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[1] See C. W. Valentine, *The Psychology of Early Childhood*, chaps. V, VI, and VIII. The relevant mechanisms mature much earlier than is commonly supposed. Valentine emphasizes " the extraordinary rapidity with which vision begins to function efficiently; sight is the supremely active exploring sense during the first few weeks and months ". In his diary of the early life of " the boy B " he notes: " Day 1. Born 3 a.m.; 10 a.m. coordination of eyes seems perfect . . . Day 2. Appears to follow my hand moving to and fro about a foot from the eyes . . . Day 5. Follows a candle-light about 18 inches from face when moved in a horizontal direction . . . Day 10. Turns his head to look at me." By about this age, the way in which the infant's movements were begun, abandoned, or recommenced, seemed to indicate that they were no mere reflex reactions, but ' voluntary movements ', due to interest in the objects as such. The coordination of eye and hand matures a little later; but by the third month definite attempts were made to reach out and grasp an object that is moved about. The dates recorded for different children or by different observers vary appreciably; but the variations seem to depend on the rates of maturation rather than on the opportunities for experience or for the gradual acquisition of associations. Gesell, in a well known investigation on a pair of twins, systematically trained one of them in discriminating, grasping, and manipulating cubes and similar small objects over a period of six weeks, while the other was allowed no such facilities: at the age of 12 months, when the period of practice was completed, it was found that the behaviour of both was virtually the same. We cannot of course say for certain what precisely the infant sees; but its whole behaviour as described by these observers, and as we can observe it for ourselves, suggests the description given in the text. If we accept McDougall's conception of ' instinctive mechanisms ' with a cognitive as well as a motor function, that after all is what we should expect.

[2] Fulke Greville, *Treatie of Humane Learning* (1633).

From this final change in formulation a number of decidedly paradoxical consequences ensue. With the original statement—"I see, or seem to see, a coin"—the qualities perceived (colour, shape, solidity, heaviness, etc.) can be plausibly attributed to a physical object. Even for those who insist on identifying the object perceived with the material object as conceived by the physicist (namely, a particular assemblage of molecules), there is no logical incompatibility between the properties attributed to the coin by the physicist and the properties the ordinary man attributes to it in virtue of his perceptions: the physicist's coin could have both sets of properties, even though the physicist as such is not interested in what he calls 'secondary qualities'. But the properties of the 'seeming-coin'—i.e. of the 'appearance'—could not possibly be properties of the physical object. For the 'seeming-coin' and its attributes exist only so long as they are actually perceived; they are essentially 'private', whereas the material coin and its attributes are essentially 'public'. Moreover, the 'seeming-coin' which my housekeeper perceived when she discovered the sovereign and brought it into my study must have been numerically different from the 'seeming-coin' which I perceive. And yet, for all practical purposes, they must have been replicas of each other, for ordinarily we should draw no distinction between them.

Now, as an account of normal perception, the version which our critic offers is (I submit) quite incredible. Indeed, when we come to scrutinize the last step in the argument, it becomes plain that a fallacy of equivocation lurks beneath the proposed reformulation. When I tell you that "what I see *seems* to be a coin" I am not trying to explain that I am just seeing the appearance of a coin—just looking at its look, so to speak. All that I mean is that "what I see is *probably* a coin"; in other words, that "I *may* be the victim of an illusion or hallucination, but in the circumstances this is highly unlikely". We must therefore carefully distinguish between the two types of case—the normal, in which my thoughts are fixed on the object and I take its reality for granted, and the exceptional, in which my attention shifts to the appearance and I regard the reality of the apparent object as questionable.

II. ANALYSIS OF NORMAL PERCEPTION

The Confusion of Problems. The philosophers who argue along the lines I have just described are, to my mind, not only jumbling together very different types of situation, they are also dealing with several different problems at once. There is first the epistemological problem—what is the correct analysis of perception as a normal form of cognition? There is secondly the ontological problem—what is the evidence for the independent existence of physical objects such as are postulated alike by modern science and by common sense? And thirdly, there is the psychological problem—what is the precise nature of the processes classed together under the rather vague names of sensation and sense-perception when those processes are studied in and for themselves.

To begin with, then, we must rid ourselves of the common assumption that there is only one correct analysis of perception applicable to all types of perceptual process. "If", says Professor Broad, "we want to clear up the meaning of some commonly used concept, it is enormously important to see how it applies to exceptional cases."[1] Now I maintain that to test a concept that has been devised to account for normal cases by applying it to abnormal cases may be gravely misleading. In an exceptional case perception itself may be exceptional. Let us then consider the normal type of perception first of all.

Perception[2] I regard as essentially a *cognitive* relation, in the sense that it yields knowledge. Under normal conditions the statement "I see a square sheet of white paper" means "There is a sheet of paper, and I see it as a white square object". In introspection I can distinguish the quality of whiteness as something that I sense; yet at the same time that very quality seems to imply an independent object with a certain size and shape of which it is an attribute. Thus, I directly cognize the white colour, and I indirectly cognize the sheet of paper—the object which I am said to 'perceive'. I can express this by saying I am aware *of* the white colour, and know *that* the sheet of paper is there. (The term 'awareness' I use to denote a unique kind of cognitive relation, namely, the relation which is common to all such processes as 'seeing', 'hearing', 'feeling', 'tasting', etc.) This distinction (as I shall try to show later) does not mean that the paper is a mere hypothetical entity *inferred* from my awareness of the sense-datum. I am still in cognitive contact with the object; but here the relation is, so to speak, a compound relation. At this stage of the analysis it could perhaps best be expressed by saying, "The paper appears as a white square to me" ('appear' being used in the sense indicated above).

It is, however, important to recognize that in ordinary life we do not just "see a piece of paper" or "see a penny" or "see a dog". The 'event' of seeing is not an isolated occurrence; it has a context. Nor are the paper, the penny, and the dog just isolated objects; they have perceptible backgrounds, they emerge ready-made out of an ever-changing landscape of which we are always more or less vaguely conscious. Russell, as we have noted, seeks to reduce all such 'events' to terms of mere 'sense-data'; Broad would reduce them to terms of 'perceptual situations'. But the way both these philosophers use the plural is somewhat misleading. What precisely is to count as a single sense-datum or as one perceptual situation? The very phrases suggest an atomistic view of consciousness such as the psychologist has long ago discarded, and, in so doing, they introduce a number of pseudo-problems which the authors have then to solve. Our visual 'data' are merely distinguishable items in one continuous visual panorama; and our visual 'sensa' merge with our various tactile, auditory, and kinaesthetic sensa, and to a large extent get their interpretation through actual movement. When Russell hears his dog barking and decides to take it for a walk, he does not (we may be sure) imagine that what he takes with him is just "a coloured canoid patch". And when I see a coin lying unexpectedly on the table or the counter, I do not pause, and say to myself "If this is not a hallucination or a trick, I judge that what I see is a sixpence," and pass on. I pick the coin up and put it in my pocket or hand it to the owner; and here again the 'perceptual situation' includes a whole system of experiences simultaneously supplied by a number of different senses, as well as the non-cognitive processes of surprise and consequent activity. It is only when the philosopher, sitting immobile in his chair, invites us to consider a single sensum, as though it were something existing in a vacuum, that we begin to wonder how such a thing can possibly yield any knowledge of a physical object. And then, sure enough, the sense-datum theory seems to follow irresistibly from these question-begging phrases.

[1] 'Critical and Speculative Philosophy', *Contemporary British Philosophy*, 1924, pp. 90f.

[2] This convenient use of the term 'perception' to denote a sensory experience with an objective reference as distinct from mere 'sensation' is due to Thomas Reid (*Inquiry into the Human Mind on the Principles of Common Sense*, 1764). In Ciceronian Latin *per-cipere* covers any form of mental 'grasping'—through feeling, observation, learning, or comprehension. The distinction itself, however, is as old as Aristotle: "I see something white", he says, "which I indirectly perceive to be the son of Diares" (κατὰ συμβεβηκὸς αἰσθητόν. *De Anima*, 418.a.21); and one of the functions of the *sensus communis* (κοινὴ αἴσθησις) is to mediate the simultaneous perception of two ensory qualities.

The position is much the same in the case of the scientist's physical objects. " What ", asks Russell, " does the scientist learn from his observations and experiments? Nothing ", he replies, " so far as physics is concerned, except immediate data of sense—certain patches of colour, sounds, smells, etc."[1] On the contrary, I would say, he learns from each of his various observations and experiments a pragmatic justification—cumulative in its strength—for the common-sense assumption of an actual physical world, revealed, however imperfectly, in our ordinary perceptions. Consider, for example, one of the oldest and most typical of experiments—the attempt to ascertain the specific gravity of a metal object by weighing it in water. When carrying out his observations and making his measurements no physicist thinks of the object he is testing or the apparatus that he is using (the piece of metal, the water, the balance, and the weights) as mere ' patches of colour ': that would make nonsense of his inferences. Or again, when photographing the tracks of colliding particles within a hydrogen bubble chamber in his quest for the basic constituents of the atom, he still regards the instruments in his laboratory and the hypothetical K-mesons, Π-mesons, and Ξ-particles that cause the visible tracks as concrete and independent ' things '—' things ' in the same sense as that adopted by the common run of men when they too use the word. The actual observations which the scientist makes to verify a scientific theory are always observations which are described or expressed in terms of everyday language and concepts, somewhat amplified no doubt or refined for purposes of greater precision, but still essentially a ' thing-language '.[2]

In every branch of science the world we seek to understand and explain is the familiar world of common sense, not a mere aggregate consisting of ' the immediate data of sense '. Some may doubt whether it is ever possible for a human being to know anything of the true character of reality. But, if we wish to gain some inkling of its nature, the nearest approach we possess is not a collection of sense data nor the mathematical abstractions that the physicist uses for formal deduction and prediction, but the concrete world of everyday life: that after all is the most real thing we know.

And yet, it may be said, even though the world of daily life is not a mere medley of " patches of colour, sounds, smells, etc. ", it is still a world of percepts, and to that extent itself a sensory datum, though not perhaps a ' sensum '. Before we can answer this, it will be helpful to introduce a modified terminology so as to avoid some of the confusion to which the prevailing versions of the sense-datum theory are all too prone. The customary phrases ' data of sense ' or ' sensory data ' are ambiguous. They cover two distinct conceptions—not only what Broad has called ' sensa ', but also what by analogy I shall call ' percepta '. I use the former to denote the epistemological object of sensation, and the latter to denote the epistemological object of perception. Thus, when I count out my change to pay a taxi-driver, what I attend to are not mere ' sensa ', i.e. just patches of colour, but ' percepta '. Having made this distinction explicit, we can now deal with the tantalizing puzzles that so easily arise when the distinction is ignored.

III. THE ANALYSIS OF ABNORMAL PERCEPTION

The Study of Exceptional Cases. Problems of this sort occur most frequently when we turn from the humdrum perceptual situations of everyday life to the more unusual or abnormal situations in which perception seems to suffer a kind of impediment or check—particularly those in which there are grounds for doubting some judgement that the perception would normally suggest or those which we have deliberately contrived in order to study some special aspect of the total process. In such situations, as I have elsewhere pointed out (e.g., this *Journal*, XIV, p. 178), *the direction of our attention automatically shifts*: instead of being concentrated on the object, it is focused on some particular sensory quality or characteristic. The varying interpretations that we can impose on intentionally ambiguous drawings—Necker's cubes, puzzle pictures, and the like—provide familiar illustrations of the surprising effects of such a change. What we at first sight perceive as the outline of a cube in three dimensional space can, with an appropriate re-direction of attention, be seen as a hexagonal diagram on a flat surface. In laboratory experiments on sensory discrimination, where the observer is required to compare spots of light or gleams of colour in a dark-room, or (it may be) notes of different pitch in a sound-proof chamber, he finds it relatively easy to detach the selected sensa from the general background of reality, because for the time being the background is so featureless; and he can then observe and examine their distinctive characteristics in and for themselves. Similarly, in certain introspective states, the sensum seems to shake loose from the everyday world of things, and to stand out as a self-subsistent *tertium quid*; and an appreciable effort may be needed to put it back, as it were, and reconstruct the perceptual whole.

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[1] ' The Relation of Sense-data to Physics ', in *Mysticism and Logic*, 1925, p. 145.

[2] This view is perhaps most clearly stated by W. Heisenberg: see, for instance, his *Physics and Philosophy*, 1958. The *theories*, of course, may be expressed in terms of highly technical concepts; but they only gain meaning and credibility so far as they can be related to ordinary everyday experience.

Under normal conditions, however, as we go about our daily business, this mental detachment hardly ever happens. It then seems absurdly far-fetched to think of a normal *perceptum* as built up out of isolated *sensa*, much as a house is built of bricks or a patchwork quilt out of many coloured bits of silk. In ordinary life the so-called *sensum* is merely an aspect or element that we discriminate within the *perceptum*; and the perceptum itself is always prior to the sensum, not vice versa. Indeed, to examine a sensum is a far harder task than to examine a perceptum. To inspect and accurately report on the bare sensory appearances demands a good deal of preliminary training. It calls for a special type of skill, such as painters and psychologists, who are practised in such tasks, have had to acquire: in observing their own sensa they are able to discern intrinsic and detailed characteristics most of which entirely escape the notice of the man in the street. There is the well-known story of the lady who told Turner that she had never been able to see the wonderful colours and patterns in ordinary objects that he depicted on his canvases. "No, madam, but don't you wish you could?" was the reply. The aim of the psychologist is to study sensory characteristics in abstraction from the concrete thing; and, in so doing, it is perhaps natural, or at least convenient, to consider them as entities existing more or less in their own right—in other words, as independent sensa. But is it legitimate to continue treating them in this way when we turn from the study of the simple sensum to the study of the complex perceptum, which, whatever else it may be, appears to form a unique and indissoluble whole?

Dr. Atkins thinks it is not only legitimate but highly plausible (see p. 166 above). Let us therefore glance at the further arguments that he brings forward in support of his view.

Illusions and Hallucinations. His first argument is essentially a re-formulation of one already brought forward by other writers, notably Price and Broad, in support of the sense datum theory. The gist of it is that, in both abnormal and normal situations, what I myself have described as 'intuitive perceptions' are sometimes found to be false; hence they may always be false. The drunkard, we are told,[1] sees 'pink rats' in his delirium. The hypnotized subject sees the body of an absent person sitting in an empty chair; he hears his voice; he feels the man's hand in his own as he says farewell. I have myself taken my reflection in a mirror at the end of a dark corridor to be an unknown stranger advancing to meet me. And, like Dr. Atkins, I have more than once been tricked by a dummy fruit—a strawberry that turned out to be a coloured lump of marzipan. Therefore I am perfectly ready to concede that in these rather special cases, when the usual tests have failed to confirm the existence of the apparent objects, we must acknowledge that

[1] I apologize for these stale reminders of familiar facts. But it is essential to examine them *de novo* to show how the natural psychological interpretation of such phenomena differs from that of the philosophical theorist.

the perceptions are delusive. But surely what occurs in these admittedly exceptional and abnormal situations is insufficient to demonstrate that, in ordinary and normal circumstances, when our perceptions *can* be fully verified, we are still mistaken in our belief that then our perceptions are almost certainly true.

However, the argument is so frequently accepted as conclusive that it is perhaps worth while scrutinizing it rather more closely to see where precisely the fallacy lies. As Dr. Atkins states it, the argument runs as follows. The drunkard's pink rat is a hallucinatory image, like the pink after-sensation I see when I have been staring at a bright green patch. Dr. Atkins calls both of them *sensa*; and points out that veridical and non-veridical sensa display no intrinsic differences such as might enable us to discriminate one type of experience from the other. Hence, he argues, if our sensa are delusive in one set of cases, they may be equally delusive in all. Consequently, when, as we believe, we see a ' real ' rat running along the water's edge, that too may be hallucinatory—part of an interminable hoax inflicted on us by our sense-organs and our brains.

Now first of all, let us note that the pink rat, the confectioner's strawberry, the person seen in the mirror or in the empty chair, are not mere *sensa*, as the after-sensation is; they are *percepta*. Therefore, unless the term sensum is given an extended meaning, these phenomena do not in fact support the ' sensum theory '.

But let us waive this verbal point, and for the sake of argument let us accept Dr. Atkins' assumption that what is experienced are in such cases *sensa*. Can we also accept his assurances about the intrinsic similarity of all sensory experiences, whether genuine or hallucinatory? Only, I should maintain, so long as we are prepared to regard each experience as an isolated sensum, occurring, so to speak, in the void. But no sensory experience ever does occur in absolute isolation, as a separate and self-subsistent event. The sensum is part of a sense-field; and sense-fields are parts of a prolonged sense-history which make up the percipient's life. Each of them has a before and an after. And it is the incongruity of a particular experience with what precedes or follows that commonly leads the percipient to reject it as nothing more than a passing illusion.

But Dr. Atkins' whole argument is really self-contradictory. It begins with the statement that certain relatively rare experiences are by general consent non-veridical; it ends by concluding that *all* our experiences are, in fact, non-veridical. Because our senses sometimes deceive us, therefore they may always deceive us. Because the finger I see in the mirror does not exist, therefore none of my fingers exist; their *esse* is only *percipi*. But plainly on such a basis terms like ' veridical ' and ' non-veridical ' cease to have any meaning.

The true interpretation of these exceptional phenomena is simple. What we call the fallibility of the senses is merely a signal instance of the fallibility of all inductive reasoning. On a number of particular occasions in the past, when I have seen an object of a particular shape, colour, and surface texture, I have, on testing it, found that it corresponds with what I have learnt to call a strawberry. I conclude that all similar objects are strawberries. And, when some practical joker presents me with a realistic replica made of marzipan, I conclude (or act as if I concluded) that it too is a strawberry. But it is the essence of an inductive conclusion that it holds good only in the majority of cases, and may easily break down in exceptional cases. The ' principle of the exceptional case ', therefore, proves exactly the opposite of what it is alleged to prove. Just because it turns out to be exceptional, it really proves the rule.

The Apparent Abnormalities of Normal Perception. What Dr. Atkins calls ' commonplace distortions ' provide a more plausible argument. Ordinarily, when I look at a penny as I twist it about in my hand, what I see, so he says, is " a series of brown elliptical shapes, varying greatly in width "; and yet I believe that the ' real ' penny is circular. Here, since the circular shape is only one of many shapes I see, and indeed itself the most exceptional, an explanation in terms of inductive reasoning will no longer apply. Nevertheless, as a psychologist, I venture to maintain that the prevailing account of everyday perception, advanced for the most part by those philosophers who insist on the falsity of naïve realism—such as Russell, Broad, Price, and their various followers—is entirely mistaken; it rests on an unconscious presumption in favour of the sensum-theory, not on impartial observation. Moreover, in the practical affairs of daily life even the severest critics of common sense seem, when one meets them in the flesh, to adopt automatically the common sense view, and, in Coleridge's phrase, to "suspend for a while their own disbelief". Thus, when in their professional moments, they repudiate the essential assumptions of plain common sense, they are in fact indulging—no doubt with perfect sincerity—in an unconscious form of double thinking.

Let me then ask the reader once again to pick up a coin, and forgetting all he has read or learnt of the subject, to turn it around before his eyes at a distance of about a foot, and say frankly what he sees. He will, I think, admit that what he actually sees is a circular disc of a certain thickness held at a varying angle by a solid-looking hand in three-dimensional space; and, unless he is already prejudiced by some cherished theory, he will also agree that ordinarily he does *not* perceive a flat elliptical patch whose shape varies with the angle.[1] If I show a young child a circular object that he has never seen before, and ask him to draw it, what he produces—or tries to produce—is still something circular. It needs a peculiar effort, a decidedly sophisticated attitude, and some theoretical acquaintance with the principles of descriptive geometry, to sketch such an object precisely as a photographic camera would portray it. If I want to see the things in front of me—the books and papers, the pen-tray and the paper-rack, the lamp and the ink-bottle—not stereoscopically as solid objects on a solid table in three-dimensional space, but as flat and coloured expanses in a two-dimensional plane, I have to strain my mode of apperception to the utmost. I find it helps to close one eye, and to *imagine* how they would look if outlined on the picture-plane. But even so, I never quite succeed. Ruskin brings the facts out very clearly in his *Elements of Drawing*, when he tells us that the main difficulty of the beginner arises, not from the manual delineation with pencil and paper of the objects he wants to depict, but from the preliminary process of seeing them as they should appear when drawn in correct perspective: we have first, he says, to "recover the *innocence of the eye*" (his italics) "that is to say, a perception of flat stains of colour, merely as such, without attention to what they signify".[2]

In our unsophisticated moments, however, when—as is most commonly the case—our attention is fixed not on the sensum but on the object, what we see is the object in a standard position and with a standard shape. These are the position and the shape which yield (*a*) the simplest possible outline, and (*b*) the greatest amount of detail. We do this (or rather the built-in mechanisms of our nervous system do it for us) not by a process analogous to inductive inference but a process analogous to factor analysis—i.e. the kind of analysis that reduces a matrix of multiple elements to its canonical form. But, as I have discussed this suggestion in some detail elsewhere, there is no need to expatiate further on it here.

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[1] In questioning my own students, when they first come to me, I find that, of students in the Arts Faculty, only 9 per cent say that they see the penny as elliptical, whereas of students in the Faculty of Science as many as 28 per cent make this claim; several of the latter go on spontaneously to argue that "of course it *must* look oval, because . . ." (and they give their own version of the fact that the image on the retina is an oblique projection of a circle, and therefore elliptical): in other words, it is clear that they are not reporting the results of an unbiased inspection, but the conclusions of an intellectual inference. Among men of business whom I have questioned only 7 per cent describe the penny as elliptical. Asked to sit before a rectangular table with a rectangular box resting upon it, and to rule out or draw what he saw, no student produced a drawing in which all three vanishing points were (by implication) consistently placed. Nor did their results entirely conform with findings of experimental studies of 'object constancy'. It is significant that in an earlier volume Professor Broad himself seems to acknowledge the point that is emphasized in the text (see *Scientific Thought*, p. 246); yet he fails to see that this largely removes one of the main arguments for the sensum-theory as ordinarily stated.

[2] *Loc. cit.*, p. 5. He suggests several exercises to help the process—e.g., looking at the objects to be drawn through a pane of glass, set in a frame, with the eye at a fixed point, and then carefully *tracing* the outlines with body-colour on the glass (*ibid.*, pp. xxiii–xxiv).

The sense-datum theorist, in order to support his conclusion, assumes that we always see things according to the laws of perspective.[1] In point of fact, we hardly ever do. When I watch a friend approaching me in the road, I do not see him getting larger and larger. Professor Schwabe has told me that it is exceedingly difficult even for the art student to depict the scene before him in true perspective. And, as is well known, when at last it has been drawn in conformity with strict theoretical principles, there is a curiously unreal air about the whole result. As Ruskin observes, few of the greatest painters ever troubled themselves about so-called linear perspective, and most of them were ignorant of its laws: " Turner, though Professor of Perspective at the Royal Academy, did not know what he professed, and never drew a building in *true* geometrical perspective in his life ."[2]

The inferences that Dr. Atkins draws from the examples given by Boring and Langfeld[3] —the man or the streetcar increasing in size as they approach and diminishing as they recede—arise from the misleading practice of singling out a particular sensum, and treating it in isolation from its context. This and most of the other well-known illustrations occur chiefly in the case of the distance senses; and the experiences we obtain from them necessarily depend, not only on the nature of the distant object so perceived, but on its spatial relation to the observer and to the observer's implicit frame of reference and on the conditions in the intervening media. Thus, what we see when we look at a person from a distance is, not a small man, but a distant man. If visual consistency were preserved in the way Dr. Atkins requires, and if what we saw was always exactly the same percept, of the same relative size, no matter how the distance varied, then an enemy near at hand would be indistinguishable from an enemy in the distance. Perceptual invariance may seem a satisfactory ideal to the theoretical philosopher; but, in the struggle for existence, it would have made not for survival but for extinction.[1]

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[1] Cf. Dr. Atkins' remarks, p. 166 sup.; also Broad, *Scientific Thought*, pp. 237f., and Price, *Perception*, p. 207. A clear formulation of the argument is given by P. J. Hirst, *Problems of Perception*, p. 46. For the matrix interpretation see Burt, *Psychometrika*, III, pp. 160f.

[2] If I understand Dr. Atkins rightly, his view seems to be that, because the image formed on the retina is analogous to that formed on the screen of a camera, therefore we first see the scene in front of us as a two-dimensional picture distorted in accordance with the laws of perspective, and then correct it for these distortions. But this, if I may say so, seems almost as naïve as the notion that, because the image formed on the retina is inverted, therefore we first see the world upside down, and then correct it for the inversion. If we are to talk (as he would have us do) in terms of a ' generation theory ', there is no reason why the sensory picture which is 'generated' by the sensory areas of the brain should resemble the optical picture formed at the back of the eye: the correspondence is topological not topographical. And the visual percept which is finally 'generated ' by the brain is a highly complex product whose nature is determined by many other cerebral processes besides those directly resulting from retinal stimulation—e.g., by the processes that result from the momentary position and orientation of my body, from the movements of my eyes, head, and limbs, from my past experience of similar objects, and by countless other ' associations' (as we loosely call them). Distortions due to perspective, though the most familiar, are by no means the only distortions to which the retinal image is subject. The effects of spherical aberration, chromatic aberration, unevenness in the retina, and the fact that the fovea lies lightly to the side of the optical axis of the visual apparatus—all these are optical effects of which we are no more aware than we are of retinal inversion, or (I suggest) of 'perspectival distortion' until we have heard or read about it. The automatic way in which, as a result of actual experience, we are able to allow for still more erratic disturbances of the normal retinal image has been demonstrated, not only by the well known experiments published by G. M. Stratton (of California) sixty years ago, but the still more ingenious experiments recently carried out by Gibson (at Smith College) and by Erismann, Kohler, and Hajor (at Innsbruck) with prismatic and chromatic spectacles and with specially constructed goggles which invert the visual field, or transpose it from right to left, or direct the subject's vision backwards. On the other hand, the eye (or rather the eye-and-brain) apparently cannot get adapted to artificial colour-stereo effects. The general conclusion seems to be that what is eventually perceived depends, not merely on the retinal image, but on the regularity of the changes involved, and above all on the general context in which the visual experiences occur.

[3] In his quotation he omits their concluding words "you see this increase in size as a movement of approach ". But I agree that the accounts given in the ordinary textbook, literally accepted, might seem to support his view.

In reaching his final conclusion Dr. Atkins makes two further assumptions. First, he assumes that there are two, and only two, mutually exclusive alternatives: either (i) *all* our perceptions *must* be wholly objective and accurate in regard to their content, or (ii) *all* our perceptions *must* be wholly subjective and therefore non-informative: the realist's theory is supposed to maintain the former; the sensum theory, as expounded by Dr. Atkins and those who think with him, maintains the latter. Secondly, he assumes that, because in certain exceptional circumstances what we immediately apprehend is a sensum, therefore we cannot on other occasions immediately apprehend a physical object. Both these assumptions seem to me quite baseless. Nevertheless, they are so frequently made that they deserve a brief discussion.

Biological Considerations. To begin with, in the natural sciences it is quite indefensible to put forward assumptions or conclusions containing the word 'must'. In psychology we are concerned solely with the interpretation of empirical phenomena. Hence we can only venture to assert probabilities. The most that I should dare to claim is that the *majority* of our perceptions are objective, i.e. literally veridical—those, namely, that occur under ordinary, normal conditions. Others may be more or less illusory or distorted. But the occasional illusions or distortions, I should hold, can readily be accounted for by the fact that the accuracy of our perceptions, like the accuracy of all our activities, physical or mental, is essentially dependent on the apparatus of brain and sense-organs with which nature has equipped us. This apparatus is the product of a long process of biological evolution operating by the rough and ready means of natural selection. We are primarily percipient animals; and all that natural selection can guarantee is that, under normal or standard conditions, the general run of our perceptions shall be sufficiently trustworthy for the practical needs of terrestrial survival. That being so, we can hardly be surprised if from time to time some of our perceptions—those, for instance, that occur during abnormal states of the physiological mechanism or under exceptional conditions in the environment—are inaccurate or even misleading.

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[1] The same applies in the case of other illusions to which Dr. Atkins does not himself refer—e.g., the rather more complicated case of an oar seen partly in water: here what we see is not just an oar which deceptively looks bent, but part of an oar in air and part in water, viewed at a definite angle; consequently the *whole* percept *ought* to appear bent. Similarly, when I look at white paper in a red light (or through red spectacles) what I see is not just the paper, but the paper-at-a-given-distance-with-a-given-illumination.

Still more striking is the way in which our perceptions are moulded and modified by affective or conative influences and by various social factors. Of two similar boxes, exactly the same in size, children very often see the one they want (a box known to contain sweets or money) as distinctly larger. The apperception of ambiguous pictures and of the indeterminate figures used in projection tests is, as is well known, strongly affected by emotional tendencies. Outside the laboratory the most familiar instances are those which Ruskin has christened for us: "Strong feeling may produce in us a falseness in our impressions of external things which I would characterize as the 'Pathetic Fallacy'" (*Modern Painters*, Pt. IV, ch. xii)—a weakness which the romantic painters and poets love to exploit with their 'blushing roses', 'tearful, tender passion-flowers', and 'cruel crawling foam', or the grim trees in the wild landscapes of *ce damné Salvator*—

> Huge trunks, and each particular branch a growth
> Of twisted fibres serpentine
> Upcoiling, and inveterately convolved,
> Nor uninform'd with phantasy, and looks
> That threaten the profane.

But, to my mind, the chief weakness of the realist position springs, not merely from the fact that illusions sometimes occur, but rather from the fact that both the biological and physiological arguments, which I have invoked to account for them, can be twisted round so as to cut in the opposite direction.

1. The critic of the realist's conclusions might, I fancy, plausibly contend that survival under primitive conditions does not depend on our perceiving the *intrinsic nature* of the objects around us—on our seeing colours or hearing sounds *because* the objects are really coloured and emit real sounds; it depends on our perceiving certain spatio-temporal characteristics which are of much greater practical relevance in the struggle for life. In order to survive, all that we require is that what we perceive should correspond with what exists, not as regards its intrinsic qualities, but merely in respect of certain abstract relations and correlations. What matters is not that we should see edible fruits or nuts in their true tints and shades, but merely that we should be able to recognize what they are and where they are—not that the things we find and eat should really have this particular odour or that particular taste, but merely that we should possess distinctive clues as to what is really nutritious and what is injurious. Thus, as the experimenters who wear chromatic spectacles are so fond of reminding us, the true colours of the world around might be quite different from those which you see or I see. We may in fact be living

In a land where the sky is as red as the grass,
And the sun is as blue as the rain.

Or quite possibly the physical world may be destitute of colour altogether (as many physicists have maintained), and all sensory qualities wholly secondary and subjective.

Consider, for example, the familiar facts of colour-mixture. If in accordance with our textbooks we regard the physical stimulus as consisting in electromagnetic radiations emitted by, or reflected from, the objects that our eyes observe, then we encounter many strange disparities between the 'physical causes' and the 'mental effects'. It is true that each particular wave-length corresponds with a particular hue, but each particular hue does not necessarily correspond with a particular wave-length; it can be produced by a wide variety of mixtures, and in some cases it corresponds with no particular wave-length at all: (e.g., the crimsons, mauves, and purples which join the two ends of the visible series into a closed circle). Secondly, by keeping to three appropriate wave-lengths only, and mixing them in the appropriate proportions, we can reproduce any visible hue we like: in the physical series there is nothing that answers to the psychological phenomenon of three 'primaries'. And finally, in the mixing of physical wave-lengths there is nothing that corresponds with the visual production of colourless grey by mixing socalled complementary colours. All of this, of course, does not suffice to *disprove* the claim that colours in some sense or other belong to the external world independently of all perception; but it does render such an assumption a needless complication for the research psychologist, whether or not we regard it as a necessary assumption for the metaphysician or the epistemologist, or for the purposes of everyday life.

2. The objections which weighed most strongly with earlier critics of the realist theory arose principally from the fact that sensory qualities themselves seem to depend, not on the properties of the object perceived, but on the physiological activities of different parts of the cerebral cortex. The evidence is too familiar to call for detailed discussion. Many people have, like myself, frequently mistaken the noise caused by slightly pathological stimulation of the inner ear for the whirring of a passing aeroplane, or the flashes caused by ocular disturbances for lightning seen out of the corner of the eye. Equally vivid flashes and stars may be caused by a blow on the head; and Penfield has described the elaborate sensory experiences reported by his patients when the cortex is electrically stimulated without an anaesthetic. After-sensations have all the characteristics commonly ascribed to sense-data. With some people visual, auditory, or tactile images are said to be almost as life-like as an actual percept. Dreams, while they last, can be still more vivid.

Of late special interest has been aroused by the phenomena produced by various hallucinogenic drugs, many of which seem able to generate sensory experiences with all the vividness of genuine reality. And finally, in certain moods of introvertive reverie, it is by no means difficult to lose all sense of reality, and imagine that one's consciousness is itself just a meaningless shimmer drifting off from one's brain like a phosphorescent mist, and fading away into nothingness as one drops off to sleep.

The inference commonly drawn that the necessary and sufficient conditions for the occurrence of a sensum, and for its various qualities or characteristics are to be found, not in the physical objects to which they are generally ascribed, but in the brain itself. And from this the physiological psychologist is tempted to conclude that all conscious experience is (as it has been sometimes put) " a mere emanation of the cortex "—" an incidental by-product generated by the working of the nervous system ".

Nevertheless, a closer examination will quickly show that neither of these arguments is really convincing. The most that they could prove is something very different from what at first sight they are taken to prove: namely, as Professor Broad very cautiously puts it, that " the *independently* necessary and sufficient *material* conditions " for a percipient to experience sensa are to be found in the percipient's nervous system.[1] The two qualifications need a word of explanation. If a particular state of affairs constitutes " a necessary condition *for* a necessary condition ", then the former is said to be " *dependently* necessary " and the latter " *independently* necessary "; and in normal conditions the *dependently* necessary conditions which we ascribe to an object are to be found *in that object*. The adjective ' material ' is inserted to allow for the possibility that " there may be independently necessary and sufficient conditions which are (in some sense or other) *psychical* "—e.g., as I should be inclined to maintain, a mind (or a mental ' field ') which has a capacity for that unique type of relation which I have called ' awareness '. Thus although the two arguments that I have summarized may serve to dissipate the somewhat bovine naïveté of the views attributed by philosophers to the so-called ' primitive realist ', they do not refute the more enlightened versions of the common-sense view put forward by Moore and Stout. All the difficulties which their critics raise can, I fancy, be adequately met by a slight re-formulation of the ordinary realist position.

The Realist View Restated. In the first place, both the biological and physiological arguments outlined above take for granted the existence of organisms and of brains, and treat them as physical objects, very like what we perceive them to be. Thus, the premisses from which our critics start assume the very things they seek to disprove. If the critic replies that he only describes these material entities in common-sense terms for the sake of intelligibility, and that what he really means are not perceptible objects, but physico-chemical systems of molecules with their attendant electro-magnetic fields, then we can reply that these systems are themselves purely hypothetical—just the fashionable scientific concepts of the moment—with far less claim upon our credence than the flesh-and-blood realities we directly perceive in our everyday experience.

All that the cautious realist would wish to maintain is that, in a normal perceptual situation, we are immediately aware of a part, or rather an aspect, of the physical object as it is under the conditions obtaining at the moment. We are never immediately aware of the physical object as a whole. Most of the critics' counter-arguments are valid only when urged against this latter contention. But after all no one could seriously believe that in a single glance we see all round and inside a house, or that with a single touch we can feel the inside of a wooden table as well as its surface or a portion of its surface. As I observe it from here, the building opposite is a yellow-grey structure, roughly cubical in shape; but I see only the front. Walking round it, I can see in turn the south, east and northerly aspects, going nearer, I distinguish the component stones, and, closer still, I perceive that each stone is a conglomerate of sandy particles; if I examined one of those grains with an X-ray spectrometer, I should no doubt find that it was a crystalline structure of atoms arranged in rhombohedral lattices; and I can readily imagine that each atom is made up of electrons whirling round a nucleus of neutrons and protons. All these ways of perceiving or conceiving what I call the house are perfectly consistent. But it is an essential characteristic of my neuro-sensory system, as an instrument for apprehending the world around me, that it operates selectively. At any one moment I can attend only to a limited aspect of a given object, and that too as a rule only through a single sense organ. Perception proceeds by taking the part for the whole, or rather treats the part as implying the presence of the whole and indicating its general nature.

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[1] *Op. cit.*, p. 165. Professor Broad himself holds that these conditions " are all contained in, or close to, the region of projection ".

With this modification of the realist position most of the so-called illusions and inconsistencies cease to be paradoxical anomalies. They are in fact what we should expect from the way our neuro-sensory mechanisms are constructed and placed. When the squinting child sees double, what does he see two of? Not two objects or two scenes, says the anti-realist, but two *sense-data* (by which he means *percepta*). Not two objects or two scenes, I would say, but the same scene from two different angles. Similarly, when I look at the reflection of my finger in the mirror, I am not really deluded. I do not see two fingers; I only see two aspects, front and back, of one and the same finger, viewed, by means of an artificial device, from two opposite directions.

There remain what Professor Broad has called 'wild' sensa—the grosser hallucinations and the milder sensory aberrations we all at times experience. Here we should, I think, recall the probable course of animal evolution. At an early stage in the process, we may presume, a new and peculiar activity either of the chemical substance in the neurones and synapses or of the electric fields set up around them seems, in some inscrutable fashion, to have been developed so as to mediate direct sensory awareness. To begin with, this must have been a mere proprioceptive or somatic awareness, i.e., an awareness of the condition of the organism itself. Then, stage by stage, it evolved still further so that it could also mediate various kinds of exteroceptive awareness, some of them depending on intervening media—floating particles, vibrations in the air, and the electromagnetic waves we call light. The fact that a perception is *mediated* does not prevent it from being *direct*. It certainly does not force us to suppose that the percept is a copy which somehow resembles or represents the thing perceived. Apart from the misleading inferences derived from a causal theory of sensation, I see no reason why such an awareness should not act at a distance.[1]

Nevertheless, like all the mechanisms evolved by natural selection, the perceptual mechanism has been carried only to the stage at which it acts with sufficient accuracy for the needs of self-preservation, and in abnormal states there may be, as it were, a throw-back. Hence these hallucinatory experiences are not absolutely 'wild'. They inform us—not always very efficiently perhaps—of something unusual going on within the neuro-sensory system. They are in effect reversions to the interoceptive or somatic type of awareness. Thus, if I can trust the results of incidental questioning, it would appear that with most people the stars produced by a knock on the head, and the abnormal noises produced by disturbances of the inner ear, are usually located within the skull, and that it is quite exceptional for such experiences to be erroneously attributed to the external environment. Most of the time they seem to be taken for what they really are—odd and unpleasant indications of something wrong within the organism itself.

Different Languages or Different Models? Professor Ayer has suggested a somewhat different way of solving the entire group of problems. He maintains that those who "say they are seeing a sense-datum in a case where most people would say they were seeing a material thing " are not propounding an alternative theory; they are " simply recommending a new verbal usage ". " In philosophizing about perception ", so he thinks, " it is useful to have a terminology that enables us to refer to the contents of our experiences independently of the material things they are taken to present "; and I myself would add that it is equally useful when we are psychologizing about perception. He goes on to argue that the controversies that have arisen are not, as the disputants commonly suppose, " disputes about the validity of two conflicting hypotheses, but merely about the choice of two different languages ".[1] Thus, when the anti-realist says " I never see material things, but only sense-data ", he is not " expressing a proposition about an empirical matter of fact, but only expressing a resolution about the usage of words ". On this view, therefore, the whole sense-datum theory would seem to be merely a *façon de parler*.

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[1] It is unnecessary here to refute the causal theory. Ayer's arguments put the position admirably (*The Foundations of Empirical Knowledge*, ch. IV). Perhaps I should add the 'mediation' to which I refer, though ordinarily phrased in causal terms, is really a logical mediation, in which two or more relations entail a resultant relation: precisely stated the correct form of the inference is (as Ayer puts it): "*p*, and, if *p*, then, if *q*, then also *r*".

With this, I think, the psychologist can to a certain extent agree. But there are three important points on which, as a psychologist, I should differ. First, I should not admit that the questions at issue were *purely* linguistic, and that *no* questions of fact are involved. Thus, when Dr. Atkins, following the tradition of the older atomistic school of psychology, insists that even under ordinary conditions we never see the material thing we call a penny but only a brown elliptical patch of varying shape, and that the ' real ' penny is not an object of perception at all but merely a ' mental construct '—in short, that " the sensing of the sensa comes first ", and that " the so-called percept is a subsequent quasilogical synthesis "—then I should contend that he is making a factual mistake, and substituting an incorrect armchair hypothesis for impartial first-hand description. The perception of the material things is unquestionably prior, and the notion of sensa a theoretical abstraction from the actual experience.

Secondly, I should not admit the two rival accounts are merely two alternative languages which we use to describe the same set of facts or experiences; I regard them rather as languages which describe two different interpretative *models*. The scientific version, as given by professional physicists, provides yet another model. And to some extent the different models are based on different sets of facts and different aspects of experience; each tends to ignore or to abstract from the remainder.

Thirdly, I do not regard them as ' alternatives ' in the sense that they are mutually exclusive. Professor Ayer, for example, tells us that the sense-datum terminology and the terminology of naïve realism are " mutually incompatible ", not because one is true and the other false, but rather because nobody can talk intelligently about a given situation if he uses two different languages at the same time. I, on the contrary, should maintain that, provided we remember always to prefix the necessary *Als ob*, it is quite possible to accept at one and the same time all three theoretical models, and to believe that, so far as it goes, each of them represents a genuine aspect of the truth. But equally I also suspect that none can give us the complete or exact truth about *Dinge an Sich*.

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[1] *The Foundations of Empirical Knowledge*, 1955, pp. 18–28. Cf. G. A. Paul, *Proc. Aristot. Soc.*, Supp. XV, 1936, and R. Wollheim, *ibid.*, Supp. XXVIII, 1954. The stock argument in support of this linguistic interpretation is that the issue between the realist and the sense-datum theories cannot be settled ' operationally ' (i.e. by an experimental test) but only ' verbally ' (i.e. by considering how the key-words are defined or used). Thus, several of my critics insist that "in accordance with the methodology of the empirical sciences " the only way to decide between two ' factual theories ' is to draw mutually exclusive corollaries or to predict distinctive observable results from each theory, and then verify the one set and refute the other by means of an *experimentum crucis* (Dr. Gregson's phrase); but this procedure is here inapplicable: hence, they say, the issue is merely verbal. My answer is that "the methodology of empirical science" includes other procedures besides the " operational". In science many concepts and many theories are put forward, not because they or their consequences are verifiable by further observations, but because they help to systematize and render more intelligible facts already established. Often we reject a hypothesis, not simply because no data can be cited to confirm it, but because it does not lend itself to theoretical systematization in the present state of knowledge (e.g. we hesitate to accept the hypothesis of telepathy, not because it cannot be ' operationally verified '—it has been ' operationally verified ' over and over again—but because it fails to fit in with the ' order of nature ' which most of us accept). Often we postulate an entirely unverifiable concept merely because without it we are landed in contradictions, inconsistencies, or a clutter of residual phenomena which would otherwise be unexplained. This type of argument occurs again and again in current works on astronomy and on nuclear physics: nobody in such cases contends that the theories proposed involve no more than an ' alternative language '. And, of course, it is the type of argument that has been most frequently adduced to support the concept of a ' psychical factor ' or an ' immaterial mind '.

Is the Physical Object part of the Perceptual Content? We are now in a position to answer the final question raised by Dr. Atkins and those who think with him. If I am right in my understanding of his criticisms, the fundamental point at issue between us is really this: I hold that under normal conditions perception does give us some direct acquaintance with the existence and nature of physical objects: he holds that, since it is impossible to regard the physical object as part of the ' content of the mind ', my contention must be false.

As Dr. Atkins phrases it, the reader might suppose that the view which he is criticizing maintains that the physical object is not, as I have called it, something ' external ', but is literally contained *in* the mind. That, as must now be obvious, is not the sense in which I used the word ' content '. I should prefer to speak of the ' content of the perception ' rather than the ' content of the mind '; and by the ' content of my perception ' I simply mean *what* I perceive, just as by the phrase the ' content of my belief ' I meant *what* I believe.[1] It might be better still to borrow Professor Broad's convenient phrase, and say that in my view a part (or an aspect) of the physical object does normally form an essential ingredient of the ' perceptual situation '.[2] When I lean on the corner of my table, I feel both its hardness or resistance and its angularity. And I see no reason why these features should not be, at one and the same time, characteristics of the table and characteristics that I directly perceive. Perception is a cognitive relation; and it is surely possible for *me* to be related cognitively to these specific characteristics, and for those characteristics in turn to be related predicatively to the *table*. Dr. Atkins apparently supposes that, if they are related to one thing, they cannot be related to the other. But that is rather like arguing that, because Winston Churchill is related by birth to the Spencers, he cannot be related by marriage to the Airlies.

Of course, if we are all the time tacitly assuming that sensory qualities are *generated* by the brain, we are bound to get entangled in a series of difficulties. But if we regard the brain as no more than an instrument for *mediating* them, the problem is simplified. We then have merely to suppose that, as a result of progressive evolution, our neuro-sensory systems inherit built-in mechanisms which, as soon as they mature, enable us from infancy onwards to perceive the things around us as independent objects existing outside our skin in the three-dimensional space in which we live and move and have our being.

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[1] As I used them, expressions such as 'in the mind', in contrast to 'external', are metaphorical. No spatial implications were intended. Thus, the word ' external ' was not intended to mean ' outside my body ' or ' my skin ', still less ' outside my skull or brain '; it was intended to describe a particular kind of relative independence, namely, that which certain mediaeval and German writers have expressed by saying that the object ' transcends ' experience (i.e. it transcends both the sensory process and the sensory content). But in English the term has unfortunately acquired a number of irrelevant associations, technical as well as popular; and consequently I have usually fallen back on the simpler metaphor.

[2] Broad expresses his own view by saying that " in no case does a perceptual situation contain as a constituent the physical object, even when there is such a physical object " (*op. cit.*, p. 156). As he actually words it, I should find it difficult to quarrel with such a statement, because all that it seems to mean is that the physical object *as a whole* is not constituent of the content of the perceptual situation; and that, as I have already insisted, is the view I also take (see footnote 2, p. 189 below).

IV. THE STATUS OF MATERIAL OBJECTS AND SENSA

The Status of Objects. It is not the business of psychology to discuss ontological problems. However, Dr. Atkins has argued that a proper psychological account of perception itself implies the falsity of certain 'common sense' notions[1] about material objects and their mode of existence. Hence, by way of concluding my survey, I should perhaps briefly indicate my own views of what he calls their 'metaphysical status'. The crucial question can best be stated in the words of Hume: "Why do we attribute a *continued* existence to objects, even when they are not present to the senses, and why do we suppose them to have an existence *distinct* from the mind and perception? [2]

1. In reply, the first point to make is one which Hume himself has emphasized, namely, that (as we have already seen) we do not reach our common-sense beliefs about the existence of material objects as a result of logical inference: such beliefs are, as it is sometimes put, 'instinctive'. A sensory percept, such as we attribute to the presence of an actual object, is distinguished from a mental image by "the force or liveliness with which it strikes upon the mind"[3]; it possesses a peculiar and an indescribable quality which German psychologists have called a *Realitätsgefühl*. Thus from the outset the very process of perception brings with it the tacit assumption that the object perceived exists independently of the process of perceiving it. The universal occurrence of these ineradicable convictions does not, of course, *prove* the existence of material objects; but it shifts the onus of demonstrating their non-existence on to the shoulders of the sceptic. And no sceptic has ever offered a conclusive proof that they do not exist. Hence the most probable conclusion is that this ingrained conviction is in all essential respects true.

2. Were we to accept the sense-datum theory in its usual form, then no doubt we should be bound to accept the logical corollary that Dr. Atkins deduces from it, namely, that the common-sense assumption of a world of material entities, existing independently of our perceptions, is at best mere hypothesis. That, indeed, is the solution adopted by Professor Broad. "The notion of persistent physical objects", he writes, "is logically merely a hypothesis to explain the correlations between perceptual situations" (*op. cit.*, p. 152). But, if so, it is a 'hypothesis' in a somewhat unusual sense of the term.

By a 'hypothesis' I understand one of two or more conceivable assumptions which are suggested by some kind of antecedent evidence (based either on an analogy or on a problematic inference), and which are capable, directly or indirectly, of confirmation or refutation in the light of *a posteriori* observations.[4] Now, when we are debating how to explain the nature of some *particular* perceptual experience, I would willingly concede that the existence of a material object, such as might account for it, may reasonably be described as a hypothesis in this sense: it is a hypothesis which we should seek to confirm by an inductive argument—a type of argument, however, which (as we have seen) is highly fallible,[1] and could therefore hardly yield the curious sense of certainty that we feel about the reality of what is perceived. But such a description cannot possibly apply in the case of the *general* concept of material objects. If the whole existence of an external world were a hypothesis calling for confirmation, then the alternative hypothesis should at least be conceivable. The alternative would be the ' null hypothesis ', i.e. the assumption that there is no external world of material objects, and that sense-data must contain their own explanation. But no one, save perhaps for a few phenomenalists in their official moments (if there are any phenomenalists still left), would dream of entertaining such a notion.

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[1] Dr. Atkins asks what I mean by the phrase 'common sense'. By the "views of common sense" he himself apparently understands "the crude and primitive ideas developed by our prehistoric ancestors and handed down with other semi-superstitious notions by popular speech and antique tradition". That is not what I meant. I meant views on a given subject which are common to nearly everyone who is not a sophisticated expert or a specialist in that subject—shared therefore by the educated as well as by what Berkeley called "the illiterate bulk of mankind that walk the high road of plain common sense".

[2] *A Treatise of Human Nature*, 1738, Bk. I, Pt. IV, Sect. ii (Hume's italics).

[3] Hume, *op. cit.*, Bk. I, Pt. I, sect. i.

[4] This is how the word seems to be generally used in science and in discussions on scientific methodology: see, for example, W. S. Jevons, *The Principles of Science*, or L. S. Stebbing, *An Introduction to Modern Logic.*

If we persist in maintaining that *sensa* constitute the *only* objects of cognition, then we are left with no prior analogue or model to suggest so unprecedented a concept as a world of relatively permanent entities existing beyond or behind the sense-data. Nor would there be any way of testing or confirming the hypothesis. The only way to verify the actual existence of a thing is to observe it, and our theory has already assumed that we can observe nothing but *sensa*.

3. Dr. Atkins refers to what I have said (in a rather different context) about ' extrapolation ' and ' educing correlates '; and he argues that we " invent the ideas of thinghood at a fairly early age by an imaginative application of the principle of analogy . . . Some sense-data ", he tells us, " are more permanent than others; they are independent of others; they form the regular antecedents of those others; and consequently we treat them as the cause of the others ." To re-phrase his own illustration, his argument seems to run: " *As* the relative transitory *flames* we see are to relatively permanent *combustibles* from which they arose (the wood and paper in the grate and the matches on the mantelpiece), *so* are the relative transitory visual *sensa* which we see in looking at the wood, paper, and matches to the relatively permanent *physical objects* which are the actual wood, paper, and matches of the plain man's philosophy." Visible and audible sense-data seem to be correlated with changes in tangible data; so perhaps *all* sense-data may be correlated (and therefore caused by) changes in tangible objects. " And ", he adds, " with increasing knowledge we modify our earlier hypothetical conceptions of these objects to fit in with our general experience." This might serve as a basis for the causal theory of perception, and for our changing scientific views about the detailed character of the postulated causes. But it could not serve as a basis for an inference to a " transcendent and self-subsistent world of physical entities ". Both the given relation and the given categories are quite different from the specific relation and the unique type of category which are supposed to be inferred.

The child who " at a fairly early age . . . invented " the hypothesis of an external physical world would be a genius; and the universality of the ' hypothesis ' would imply that practically *all* children were geniuses—geniuses who in every case happen to hit on the same hypothesis. We cannot suppose that the similarity is due solely to social or linguistic tradition, because it is found in isolated communities who have had no common ancestors and no communication with each other. The fact is, of course, that no one ' invented ' the idea of an external world. Objective reference is implicit from the start; it is part of the very nature of cognition to transcend itself.[2]

But the puzzle, I fancy, arises largely because in our philosophical moments we forget that we are not merely perceptual beings: we can remember as well as perceive, and we are endowed with a consciousness which is not merely cognitive, but at the same time affective and conative. All three forms or aspects of experience imply objective reference: the child who hopes to find a ripe apple on a tree, or fears there may be a bear under his bed, is thereby tacitly assuming the existence of an external world of objects. We can decide whether or not we shall look in this or that direction; but what we perceive when we do look in no way depends on our preference or activity. Frequently it seems to resist it. If Dr. Atkins declines to believe in the existence of his ' closed door ', the best reply will be Dr. Johnson's: Kick it! or Try to walk through it! Both the kick and the attempt to advance will be resisted ; for the object is also an obstacle.

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[1] See above, p. 187.

[2] Even Broad admits that " in all perceptual situations there is an external reference *beyond* the objective constituent " (*op. cit.*, p. 154; by the ' objective constituent ' he means of course, not the *material object*, but what is commonly called the ' mental content ').

4. One or two supplementary explanations seem called for in answer to Dr. Atkins' further questions. I fully agree that, when in any scientific or philosophical discussion we use such a phrase as ' physical ' or ' material object ', it is incumbent on us to indicate how it is to be understood in the arguments that follow. But this is plainly a linguistic rather than a factual problem. Hence for the purpose of such discussions we must treat the phrase ' physical object ' as designating a specific category to be defined by postulates, though the particular postulates we select may be determined by our preferences for this or that theoretical model.

Galileo and his immediate successors identified physical objects with material objects, and defined them solely in terms of their so-called ' primary qualities ' (*accidenti*), and regarded ' secondary qualities ' as " existing only in the sensitive body ".[1] Now, with the exception of taste, the secondary qualities (sound, colour, smell) are all mediated by distance-senses; and I would suggest that their survival value arises chiefly from their utility as means of *identification*; the primary qualities (shape, actual size, hardness, weight, etc.) are mediated by what Galileo called ' touch ' (which for him included kinaesthetic sensation); and these are the qualities most useful for purposes of *prediction*. Modern science tends to pare down the defining characteristics of the physical object to a still narrower core, namely, to those characteristics that are metrical, and so lend themselves to mathematical treatment. These are the characteristics which, more than any others, facilitate a totalitarian formulation and precise prediction.[2] They are all essentially quantitative;

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[1] *Il Saggiatore* (1623), Q.48. Cf. Locke, *Essay Concerning Human Understanding* (1690), Bk. II, ch. 8.

[2] As Eddington observes, many physicists and philosophers in the past have hankered after " an ideal, shining in front of them, that the whole of our knowledge of the physical world might be unified into a single science expressed in geometrical or quasi-geometrical conceptions " (*The Nature of the Physical World*, p. 136); but it is not an ideal to which he or other modern physicists would subscribe. Dr. Atkins certainly goes too far when he maintains that the special merit of a mathematical treatment is that it enables us to achieve " a single systematic formulation which shall be at once internally consistent and complete ". Like Spinoza (*Ethics*, III, Preface) he apparently wants us to " deal with human perceptions, desires, and actions exactly as if [we] were dealing with lines, planes, and bodies ", and so hopes to turn psychology into a quantitative science and remove " the inconsistencies and the incompleteness which are its crowning scandal ". Unfortunately the popular notion that by converting a branch of study into a mathematical system we shall be able sooner or later to render it at once " internally consistent and complete " is now known to be mistaken. In a celebrated paper (' Über formell unentscheidbare Sätze der *Principia Mathematica* und verwandte Systeme ', *Monatshefte f. Math. u. Phys.*, XXXVIII, 1931, pp. 173–198) Kurt Gödel published a theorem, which (expressed in a slightly modernized form) demonstrated that no formal system could be *both* complete *and* consistent: if it was complete, it would contain contradictions; if it was consistent, it would be incomplete in the sense that it would contain an unlimited number of undecidable propositions. A series of similar logical antinomies were discovered about the same time, most of them hinging on propositions that involved self-reference and/or a quasi-circularity: (the oldest and most familiar instance is the saying attributed to Epimenides the Cretan—' Everything a Cretan says is a lie ', quoted by St. Paul, *Titus* I, 12). Such paradoxes are by no means just trivial verbal anomalies—amusing, but of no further significance. Among the more enthusiastic mathematical logicians of the day these cumulative discoveries produced much the same kind of shock as the proof of the incommensurability of the side and the diagonal of a square is reported to have produced on the early Pythagoreans, who were inspired by a mathe-

qualitative characteristics are ruthlessly expelled from the party-organization, and relegated to a kind of isolation-camp where psychologists and psychiatrists are left to do what they like with them. The privileged characteristics about which practically all scientists are seemingly agreed are (i) duration in time, (ii) size, shape and position in space, and (iii) spatial relations to other objects. For the purposes of physical science each of these characteristics is defined in terms of a relation. Each, however, is analogous to, even if not identical with, a sensible characteristic which can be directly apprehended. Indeed, the whole physical universe seems to be conceived by the majority of scientists on the analogy of a visual field.

Now, in spite of the assertions of Dr. Atkins and other writers, these chrono-geometrical characteristics can hardly be the *only* ones to be retained in the scientist's abstract scheme. As Berkeley long ago pointed out, nothing can be *merely* extensible in space, or *merely* movable in space and time: anything which possesses such characteristics must also possess at least one quality capable of covering an area or filling a volume. Or, to put it in more modern terms, no ' existent ' can exist unless it is characterized by ' qualitative ' properties as well as ' relational '. Nor has it proved feasible up to the present to draw the line at just one such characteristic—for example, mass; electric charge is equally essential. And, since we have thus been obliged to add a second quasi-qualitative characteristic to our list, there is no reason we should not add more—e.g., if we so desire, colour. (Of course, these additional characteristics might be dependent upon, or predictable from, the others; but that does not of itself annul them). Even Broad, if I understand him rightly, holds that sensible secondary qualitities *may* quite possibly characterize physical objects; his objection is simply that, as things are at present, " we have no reason to think that they do ".[1]

Thus, according to the postulates we choose, we can define the so-called ' physical object ' in various ways—there is what might be called the ' perceptual object ' of everyday life, the ' material object ' of science, and the *Ding an Sich* of the metaphysicians; and we can, I suggest, harmonize their apparent inconsistencies by recognizing different levels or

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matical ideal very similar to that of Dr. Atkins. It renders his ideal of psychology as a quantitative science quite untenable. The doctrine of ' logical types ' does not entirely rescue us from these recurrent predicaments; since, even if the proposition that is undecidable in system A_1 is decidable in the meta-system A_2, that system in its turn will contain new undecidable propositions of its own. As a consequence it would now, I think, be generally agreed that none of the so-called exact sciences can produce complete and consistent accounts of their respective universes in terms of their own assumptions. Even Heisenberg's latest ' world formula ' (a title which he himself has deprecated) is admittedly subject to these limitations: as he expresses it, " to understand nature is by no means the same as to experience nature: let us strive to do both ". Thus, as so many of the older philosophers maintained and the nineteenth century romantics were never tired of emphasizing, "there is, and always will be, an irreducible element in the cosmos which bids defiance to any interpretation that is exclusively intellectual ". Hence the lack of completeness and the lack of perfect consistency with which the interactionist is charged is by no means the fatal consequence Mr. Atkins supposes.

[1] Surely, however, there is at least one obvious reason, namely, our natural conviction, based on a direct intuitive impression, that colours, etc., *do* characterize the surface of physical objects, or at least pervade the corresponding regions of space. As we have recognized all along, to accept such impressions at their face value undoubtedly gives rise to difficulties; but, as Broad very candidly owns, no theory is entirely free from difficulties. In his summing up he himself is far less dogmatic than those who, like Dr. Atkins, have championed his views. Broad's own words are: " On the whole, there are no greater objections to the Sensum Theory than to the other theories "; nor do these other theories have any positive advantage over it. Accordingly, " as the Sensum Theory does not require us to assume Absolute Space-Time, . . . the balance of advantage seems to be *slightly* on the side of the Sensum Theory "—not, I suggest, a very conclusive reason (*op. cit.*, p. 195, my italics).

degrees of reality. But it would be out of place, even had I the room, to amplify these speculations here.[1]

The Status of Sensa. When I am asked what precisely is the 'status of *sensa* or *sense-data*', I am tempted to reply that this is a problem for the sense-datum theorist. In my view it seems to involve the fallacy of 'many questions'. However, if we start with the same classification as was adopted in my previous article, then it seems clear that 'sense-data' (if there are such things) must be 'particular existents' not 'universals', i.e. substantives rather than adjectives. But in that case are they 'continuants' or are they 'occurrents' (i.e. 'events')? I myself should regard the *sensing* of a sense-datum as an 'occurrent', but *what* is sensed, no matter how brief as judged by human standards, must surely be a 'continuant'. As such it may, of course, be a more or less transitory element in a more complex and comprehensive continuant, i.e. in what is commonly called a 'substance'. Professor Price,[2] however, holds that sensa are 'events', and as such necessarily *phases*, not elements or constituents, of a 'continuant' or 'substance'. In either case, however, a further question obviously arises: of what substances are they 'phases' or constituents?

As to this, several different theories may be, and indeed have been, put forward:

1. That *sensa* are essentially *physical*, i.e. phases or elements of physical substance;
2. That *sensa* are essentially *mental*; and here I use the term 'mental' to cover three alternative possibilities:
    (*a*) that they are *psychical*, i.e. phases or elements of a mind;
    (*b*) that they are *cerebral*, i.e. phases or elements of a brain;
    (*c*) that they are *psycho-cerebral*.
3. That *sensa* are *neutral*, i.e. are neither physical nor mental;
4. That *sensa* are, so to speak, *physico-cerebro-psychical*.

The first view (1) was defended by G. E. Moore, and is the one favoured by Stout and Price.[3] Broad, however, and, I fancy, the vast majority of sense-datum theorists, reject it. (2*b*) is the view accepted, usually without argument, by most physiologists and physiological psychologists: but those who have systematically examined the implications of any such view seem almost unanimous in regarding it as untenable. They generally dismiss it with Bradley's scornful retort that "when I smell something unpleasant, it is not because I am aware of the stinking state of my own nervous system". The dualist followers of Descartes, the idealist followers of Berkeley, and most of the earlier interactionists and psychophysical parallelists have been inclined to accept (2*a*). What nowadays are called 'sensa' they have generally described as 'ideas of the mind' or 'mental states'—phrases that are treated as implying that they are both private and subjective.

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[1] See the remarks and quotations in 'The Psychology of Perception', this *Journal*, XIV, p. 177 and footnote 2. I should agree with Mr. Hutton that, "if we could regard secondary qualities (such as colour) as objective characteristics of the material universe, then it would be easier to regard *value* (such as the beauty of a landscape) as equally objective". But I should not agree that "the denial of the former entails a denial of the latter". My own conclusion would include a modest qualification; I should prefer to say that colour is an objective characteristic of the universe *as we know it*. In the same way I should say that sound and music are an objective characteristic of the universe *as we know it*; but I find it difficult to suppose that the melodies and harmonies I hear at a concert, or the colours that I see when I carry out experiments on colour-mixture, are characteristic of any specifiable physical object or substance. This does not mean that the impressions we try to describe in our theories of common sense realism are necessarily mistaken; but merely that the verbal descriptions may be misleading.

[2] *Perception*, p. 116.

[3] Cf. G. E. Moore, 'A Defence of Common Sense', *Contemporary British Philosophy* (2nd Series, 1925, pp. 193–223); and G. F. Stout, *Mind and Matter*, 1931, pp. 127f. Price is a little more cautious: he states that "in certain specifiable cases . . . visual and tactual sense-data are in some sense or other constituents of the surfaces of material objects" (*op. cit.*, p. 111).

Broad adopts the third alternative (3). Sensa, he tells us, " are particular existents of a peculiar kind ", postulated specifically by the sense-datum theory. It is true that " in having spatial characteristics, etc., they resemble physical objects; but in their privacy ∴ . they are like mental states ";[1] nevertheless they are themselves neither physical nor mental, nor yet " existentially mind-dependent " (*op. cit.*, p. 181). On this view the sensum appears to be a wholly anomalous *tertium quid*, generated simultaneously with the process of sensing it, created *ex nihilo* and vanishing *in nihilum*. Broad himself admits that " the Sensum Theory involves a very odd kind of causation, almost a creation out of nothing ". But would it not be more plausible to conclude, not that sensa are *neither* physical *nor* mental, but that they are *both* physical and mental—amphibious rather than neutral? Elsewhere, indeed, Broad maintains that what is ordinarily called the mind is a compound of two factors—a bodily or cerebral factor and a psychic factor. Accordingly, one would have expected him to regard sensa as essentially psycho-cerebral, dependent on both brain and psyche.[2] Nevertheless, under normal conditions, when the media, the brain, and what we call the mind are all in their usual state, external stimuli are needed to excite these sensory experiences, and the variations in their characteristics are correlated much more closely with variations in these external stimuli than with the relatively invariant mind or brain. And it was for this reason that I maintained, in the passage that one of my critics has queried, that " they are more objective than subjective ".

Should we not then take all three independent variables into account, and describe the dependent variable—the so-called sensum—as physico-cerebro-psychical? But this question, like the others, at least in the form in which they are worded—appears to rest on a false assumption, namely, that each sensum is a single self-contained entity; whereas, as we have repeatedly seen, it is merely a differentiation in a single continuous presentational field—one vast and incessantly changing sensum which makes up the individual's conscious life-history. If anything therefore we ought to ask, what is the 'metaphysical status' *of this continuum*?

By now, however, it must be plain that the issues raised are largely due to the inadequacies of language. Perceptual knowledge is non-verbal knowledge: and yet the scientist has to talk of it in verbal terms. Grammatically, physical objects—'things' as we popularly call them—are substantives; the perceptible qualities we ascribe to them—pink, grey, brown, circular, etc.—are adjectives predicated of those substantives. This is the syntactical framework furnished by the language of everyday life; and it is the framework generally adopted by the exponents of the sense-datum theory. Yet, when we start, as most of them do, with 'exceptional cases' rather than with the common-place situations with which most of our life is concerned, we find that the substantives presupposed (the drunkard's rats, the fingers in the mirror) are non-existent; and we are tempted to conclude that the rats, the fingers, the pennies, etc., that we see in everyday life may be non-existent too. Consequently, if we are to preserve our ingrained substantive-and-predicative habits of speech and thought, a new substantive has to be introduced which will be equally applicable to both normal and exceptional cases; and so the word *sensum* gets invented to satisfy this linguistic need.

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[1] Dr. Atkins tells us that " every psychologist and philosopher who has discussed the subject has acknowledged the fact that sensa are *necessarily* private ". This is hardly correct. Ayer, for example, states that " in asserting the privacy of sense-data one is not acknowledging an empirical fact, but laying down a verbal convention "; and he recognizes that, if we consider it convenient, we could readily bring the rules of our sense-data language into conformity with the everyday usage, whereby we all of us speak of different people perceiving the same sound and the same colour (*The Foundations of Empirical Knowledge*, pp. 153–154).

[2] This is the conclusion suggested by Professor Price (*op. cit.*, p. 133).

If we are asked how precisely we are to categorize the new concept, the answers given will depend primarily on how we, in our philosophical, psychological or practical moments, have found it convenient to define the other terms used in describing ' perceptual situations ' —mind, brain, physical object, stimulus, and so forth. To seek any final and decisive explanation of what they really are in themselves is a quest beyond human powers. And, after all, as scientists working in other spheres have so often reminded us,[1] all that science can really hope to discover is not the intrinsic nature of the things, but merely their relations.

A Psychological Model. So far as psychology is concerned, the answer to Dr. Atkins' question seems quite clear. No other branch of science makes it part of its business to investigate sensory experience as such. That indeed is an inquiry which must cut right across the divisions of other sciences, since, as we have seen, differences in sensory experience turn out to be correlated with differences in conditions which are partly physical, partly physiological, and partly mental. Moreover, sensory experience is inherently qualitative; and qualitative characteristics are expressly excluded from the phenomena investigated by physics and physiology. Hence the systematic study of sensory experiences in and for themselves becomes one of the chief responsibilities of the psychologist. This implies that, if any explicit classification is required of him, he must reply that, *qua* psychologist, he is compelled to treat, not only sensory processes or relations, but also sensory qualities or contents, as mental phenomena rather than physical.

In pursuance of his researches, his method, like that of every empirical scientist, will be partly analytic. It will begin by singling out the distinctive elements or aspects of the phenomena to be studied, and by examining each in relative isolation. Hence there can be little question that for the psychologist the appropriate hypothetical model and the appropriate linguistic terminology will be those of the sensum theory, no doubt with appreciable adjustments—chiefly modifications which to some extent embody the features emphasized by the ' multiple inherence ' and the ' multiple relation ' theories.[2] This means that, instead of a naïve subject-and-predicate language, we must change to a more comprehensive linguistic scheme which will include relations; and, in doing so, let us remember that relations can be compound as well as simple, three-or-more-termed as well as two-termed. In this way it seems possible to escape most of the paradoxical corollaries to which the sense-datum theory in its current form appears to commit us.

The analysis I myself have suggested is briefly as follows. Strictly speaking, conscious experience is a single ever-changing flux, at once complex and continuous. Within this *totum datum* I can distinguish (though I cannot separate) certain semi-independent items—in particular certain percepta, and within these percepta certain sensed qualities which under certain conditions are sufficiently distinct to be termed sensa (in the plural). And thus, ignoring any affective and conative aspects of the total conscious stream, and considering only the cognitive aspects, we are forced to recognize at least three unique types of relation:

(i) A relation of direct cognition or *awareness* between me (the ' subject ') and a sense-content. Moreover, as the phenomena of attention seem clearly to indicate, this is a varying relation (or a group of relations), and not just a single invariant relation, as is commonly assumed.[1]

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[1] E.g., A. Eddington, *The Nature of the Physical World*, pp. 230f., section on 'Relation Structure'; cf. *id.*, *New Pathways in Science*, pp. 256f; " our account of the external world must necessarily be a Jabberwocky of unknowable actors executing unknowable actions: the only knowledge we can acquire is knowledge of a pattern or relational structure contained in the actions ".

[2] As before, I should maintain that these and the sense-datum theory are not three alternative theories, but linguistic (or rather logical) models which are mutually compatible, and which, each of them, express an important aspect of the truth.

(ii) A relation, analogous to *implication*, between this sensory content and an independent existent (or 'object') which it seems to qualify. The term 'implication' must not be taken to mean that the object is inferred.[2] Whether we describe the relations as one of implying, or signifying, or belonging, will hinge partly on the nature of the actual situation, and partly on the purpose and context of the analysis we are undertaking.

(iii) A resultant or compound relation of indirect cognition or *perception* between the 'subject' (as and where he is at the moment of the analysis) and the 'object'. The words 'resultant' or 'compound' refer to the logical nature of the relation, not the factual or causal nature of the process. As a consequence we can say of the object—Professor Price's tomato, for example—that 'it appears red to me'; but the redness is not something prior to the perception, it is a particular kind of redness qualifying a particular solid object seen out there in space.

Since each of these three relations is unique and irreducible, occurring, so far as we know only in the case of entities or organisms that are conscious, the problems they present are essentially problems for the psychologist, and, as I have argued elsewhere, demand trained introspection for their study. And since sensa as such are directly accessible only in connexion with such entities or organisms, they too must be regarded as falling within the psychologist's province.

V. SUMMARY AND CONCLUSIONS

Most of the facts that I have cited are part of the stock in trade of those who have discussed the problems with which we have been concerned; and if I have re-examined well-worn examples at what some may think an inordinate length, it is not so much because some of them may be new in this context, but rather because the sense-datum theorist tends to select and concentrate solely on those particular instances that lend support to his theory, and pass the others by. Nor have I sought to present a systematic and well-rounded theory of perception, but merely to answer the specific questions put by my various critics and correspondents. That of necessity leaves my arguments somewhat loose at the joints and frayed at the edges. But to embark on a more complete and conclusive treatment would in my opinion be premature, and in any case would need not a paper, but a book. Here, therefore, I will content myself with recapitulating the main points that I have endeavoured to stress.

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[1] Thus my answer to Mr. Hutton's question about what is familiarly known as the 'problem of the speckled hen' is two-fold according to the way it arises. (i) With my reading glasses I see something vaguely dappled; with my distance glasses I see something distinctly spotted, and can almost count the spots: here I should maintain that there were *two* sensa. (ii) Suppose, however, that, while my eyes are looking at the hen, my attention is at first occupied with tomorrow's lecture, but then for some reason is suddenly focused on the hen: in this case I should say that the sensum (the visual field rather than the isolated object) is the same, but that my awareness of it changes: it is still 'immediate', but there seem to be degrees of immediacy. Once again the question is largely a matter of verbal definition. But, if we insist on the assumption of an unvarying type of awareness, we shall have to multiply the number of sensory entities to an extent which soon becomes extremely embarrassing. There are, too, other grounds which in my view force the psychologist to assume variations in the quality or clarity of the cognition; but this is not the place to discuss them in detail. Although I fully sympathize with Mr. Hutton's criticisms, I fancy he rather magnifies the one-sidedness of the different schools and the depth of the rifts between them. That, however, is a type of rhetorical exaggeration to which we are all, as controversialists, occasionally prone. In the interests of brevity and lucidity one is tempted to overemphasize the differences. Possibly, therefore, the eminent writers whom I myself have ventured to criticize would be inclined to accept a good many of the points I have made. Usually, indeed, I have been more concerned with the way students (so far as I have talked with them) commonly interpret and simplify the views of such writers, and so may sometimes have done them an injustice.

[2] This was the error in the older form of the representative theory of perception. It treated the cognition of an external object as something so indirect that it became a mode of judgement rather than a mode of awareness. Thus the observational character of perception was entirely destroyed. In my view judgement, as a rule, only arises when we apply a name, and then it is implicit rather than explicit.

1. The sense-datum theory, as stated by its more popular exponents, and as interpreted and accepted by nearly all my critics, appears to rest on a number of assumptions, which a closer psychological study shows to be untenable.

(*a*) The phrase 'sense-datum' is itself ambiguous. If we construe it in the broader way as covering any kind of 'sensory datum'—everything, that is to say, which is 'given' in cognitive experience, whether simple or complex—then it includes, and to some extent confuses, not only (i) sense-contents as such, i.e. what Professor Broad has termed *sensa*, but also (ii) what I have called *percepta*, which in my view form the objective terms of an entirely different conscious relation. The older representative theory of perception failed to differentiate clearly between the two, and what it called 'ideas' were percepta rather than sensa. The current versions of the sense-datum theory refer primarily to sensa, yet many of its premisses are plausible only if taken as including percepta as well.

(*b*) As ordinarily formulated, the theory treats isolated sensa as cognitively prior, and material objects as mental constructs synthesized from the original sensa. But the whole trend of recent psychological work has been to show that what is cognitively prior is the perception of concrete objects, and that the sensum as such is a theoretical abstraction.

2. It is a mistake to hold that there is only one analysis of perception applicable to all forms of perceptual process. In particular two types of situation must be distinguished:

(*a*) There is first what may be called the ordinary or practical type, such as occurs in everyday life where the conditions both of the environment and of the organism are approximately normal. These are the situations with which the human neuro-sensory system was evolved to cope. Here the so-called 'realist' interpretation that is implicitly adopted by common sense would seem to be adequate, once it has been purged of its needless 'naïvetés', and formulated in terms that are consistent and precise.

(*b*) There are secondly the rarer types of situation, in which the environmental and subjective conditions are abnormal or at any rate exceptional. These include, not merely the problematic cases of illusion and hallucination, but also the artificial conditions which psychologists set up (i) when attempting a purely theoretical analysis by armchair introspection, or (ii) when carrying out a controlled experiment in the laboratory to clarify certain limited aspects of the total process. In these, owing to a redirection of the observer's attention, the position is fundamentally changed. In such cases the model and the terminology provided by the sense-datum theory appear to be more suitable. But even so the theory still requires certain important modifications.

3. The apparent distortions, illusions, and hallucinations that arise in relatively exceptional circumstances do not in themselves suffice to demonstrate the unreliability or the subjectivity of our perceptions in ordinary or normal situations. They can in fact be readily explained by the unusual peculiarities of the abnormal situation itself or by the inadequacy or inefficiency of the physiological mechanisms which mediate perception.

4. The fact that the concepts and the terminology of the sense-datum theory are, at any rate in the present stage of knowledge, the most convenient for the psychologist to adopt in his theoretical analysis of the processes of perception must not be taken as entailing any specific metaphysical or ontological consequences. Appropriately formulated, the theory in no way compels us to deny that material objects may actually have the secondary qualities (colours, etc.) that they appear to have. And an impartial study of the facts of perception seems on the whole more in keeping with the common-sense view, which treats such qualities as normally forming attributes of the physical object, rather than with the more sophisticated epistemological theories, which treat them as wholly subjective.

5. The various theories need not be regarded as mutually incompatible or rival alternatives. They are essentially hypothetical models, each having its own special merits of linguistic and conceptual convenience and its own limitations, and none of them able to dispense with a certain amount of as-if-ery. All incorporate important elements or aspects of the truth. None of them—nor yet a combination of them—can be expected to give a final or complete interpretation of the Sphinx-like face of Reality.

6. None of the evidence and none of the theories furnish any cogent ground for discarding the tacit assumption which we all of us—plain men and scientists alike, whatever our philosophical creed—instinctively accept in our every day life, namely, that the direct outcome of the long and complicated chain of events, which apparently forms a necessary condition for perception, is not the mere generation of private 'sensa', but *a genuine if partial revelation of reality*, or at least of certain significant aspects of it.

OBITUARY

SIR RONALD FISHER

Ronald Aylmer Fisher was the seventh child of George Fisher, head of a well known firm of auctioneers in King Street, St. James's. He was born on 17 February, 1890, and educated at Harrow and Gonville and Caius College, Cambridge. He graduated in the mathematical tripos as a wrangler at the age of twenty-three. Owing to his extreme short sight, he was exempted from military service during the First World War; and, after a couple of years as statistician to the Mercantile and General Investment Company, he accepted a post as mathematical master at Rugby. In 1918, when only twenty-eight, he published his elaborate paper on the 'Correlation between Relatives on the Supposition of Mendelian Inheritance' (*Trans. Roy. Soc. Edin.*, LII, pp. 399–433). This has now become a classic; but at the time aroused little attention, except from one or two statisticians and psychologists who were interested both in the problems of heredity and in correlational techniques. In it Fisher boldly ventured to re-analyse Karl Pearson's data on human body measurements, and showed that, contrary to Pearson's own conclusions, the results were not in conflict, but in close agreement, with those predicted by the Mendelian theory of multifactorial inheritance.

In the following year he was appointed head of the statistical section at Rothamsted Experimental Station. Here he embarked on the remarkable series of investigations which revolutionized not only agricultural research but also statistical theory. The new techniques were described in his best known work, *Statistical Methods for Research Workers* (1925). In 1929 he was elected Fellow of the Royal Society, and awarded the Weldon Medal. The next year he published *The Genetical Theory of Natural Selection*. This dealt primarily with the Darwinian theory of evolution, and applied it more particularly to explain the rise and fall of nations and the appearance of class differences. But its main influence was due to the striking way in which he succeeded in uniting Mendelian concepts with biometric concepts—two rival modes of approach which had hitherto been regarded as incompatible and mutually exclusive.

In 1933 he succeeded Karl Pearson as Galton Professor of Eugenics at University College, London. And this is perhaps a fitting moment to record the deep debt that my research-students and I owed to him and to his staff for their kindly but critical assistance throughout the period that we were members of the same foundation. Down to about that time, the statistical techniques of psychologists both in this country and overseas, had consisted almost entirely of correlational procedures, with 'large sample formulae' for testing statistical significance. As is well known, one of Fisher's most notable achievements was the development of new methods for evaluating results obtained with small samples; and that was precisely what psychologists needed. The principles underlying analysis of variance and Fisher's schemes for systematically designing experiments furnished us with still more effective methods of research. More particularly he lent the weight of his authority and approval to encourage the tentative efforts some of us were making, in spite of the critical attitude of the behaviouristic school, to investigate mental inheritance and to study what was popularly termed the problem of 'nature and nurture'.

Fisher's lectures, like most of his writings, were by no means easy for the ordinary student. Moreover, his standards of statistical rigour were so high that he was at times almost disdainfully impatient of the cruder procedures with which the psychological amateur was too often content. Yet, in personal conversation, with his quick way of penetrating to the heart of the problem, and his amazing fund of miscellaneous knowledge, he proved both a stimulating and a sympathetic adviser.

In 1943 he accepted the Arthur Balfour Chair of Genetics at Cambridge; and some years later was also elected President of his old college. Three years ago he retired, and then began a tour of Australia as guest of the Commonwealth Scientific and Industrial Research Organisation. In the course of his visit he made up his mind that Adelaide would be an attractive place in which to settle down, and he returned thither the following year. There, on Sunday, the 29th of last July, he died in full mental vigour, at the comparatively early age of seventy-two.

C.B.

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WORKING MODELS OF THE BRAIN

Critical Notice

Zwar ist's mit der Gedanken-Fabrik
Wie mit einem Weber-Meisterstück,
Wo ein Tritt tausend Fäden regt,
Ein Schlag tausend Verbindungen schlägt.

Der Philosoph der tritt herein,
Und beweis't euch, es müsst' so sein.
Das preisen die Schüler aller Orten,
Sind aber keine Weber geworden.

Wer will was Lebendiges erkennen und beschreiben,
Sucht erst den Geist heraus zu treiben.
Dann hat er die Theile in seiner Hand,
Fehlt leider nur das geistige Band!

Goethe, *Faust*, I., l. 1921-1939.

The Brain as a Computer. By F. H. George. Oxford: Pergamon Press, 1961. Pp. 413. 63*s*.

Dr. George's aim, in his new book as in several of his recent papers, is to demonstrate the advantages of " regarding the brain as if it were a machine of the computer-type ". In doing so, he believes, we are " making explicit a view that has long been implicit in the biological sciences," and especially in neurophysiology. His plan, therefore, unlike Dr. Sluckin's in his more popular book on *Minds and Machines*, is not so much to summarize the results so far obtained or the conclusions tentatively reached by adopting this approach, but to explain the methodological principles involved. Dr. George, we have been told,[1] is one of the " two outspoken protagonists of the automaton theory in this country ". The publication of his book, therefore, provides us with an opportune occasion for considering how far the fashionable picture of the brain as a kind of electronic computer can in fact be justified.

Except for the illustrative circuit-diagrams of three early models (Ross Ashby's homeostat, Shannon's maze-runner, and Grey Walter's mechanical tortoise), his book contains no complete specifications for any ' models ' in the literal sense, i.e., electrical mechanisms that have been actually constructed and found to work. The ' models ' with which he is concerned are for the most part what Turing called ' pencil and paper machines '—i.e., symbolic diagrams with verbal explanations. What he describes, as he himself puts it, consists of " explicit theories which may be regarded as blueprints for *possible* models ". Since I shall have a good many criticisms to make both of the theories proposed and of the way they are expounded, let me say at once that every psychologist will find the volume a most valuable addition to his library. It is plainly the fruit of much industry and genuine enthusiasm. The several chapters, together with the long bibliography appended at the end, provide a mine of useful information, culled from numerous books and periodicals, many of which are by no means readily accessible to the ordinary reader.

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[1] J. Beloff, *The Existence of Mind*, 1962. The other protagonist is Dr. D. M. Mackay, to whose views I shall be referring later on.

Essentially the method which Dr. George desires to advocate consists in an endeavour to "restate the ordinary kind of biological evidence and arguments with mathematical precision", so that a working model of the hypotheses envisaged could, in principle at any rate, be constructed in actual hardware. The notion is by no means so new as is commonly supposed. In the past, so soon as any new scientific mechanism attained general popularity—whether built out of pulleys and strings, cogwheels and levers, or ingenious hydraulic, pneumatic, electrical, or electronic devices—it has almost at once been taken up and exploited to activate a working replica of a human being. Nearly two thousand years ago Hero of Alexandria in his *De Automatis* described mechanical statues of "gods and maidens that could move and utter sounds". More ambitious contrivances, of which the details are better known, were those of the early Swiss watchmakers: a clockwork mechanism was inserted into the body of a life-size doll, furnished with joints and limbs conforming to the descriptions of contemporary anatomists; and, when the springs had been wound up, the creature would jerk its legs as if walking or dancing, wave its arms in rhythmical gestures, and grind out a few simple tunes—like Olympia in the *Tales of Hoffmann*. Descartes, impressed by the ingenious fountains and waterworks that were being erected in Amsterdam, sought to formulate his theory of reflex action in terms of a hydraulic rather than mechanical model; and physiologists began talking of the 'flow of currents' up and down the nerves, and thinking in terms of pipes, pumps, and reservoirs.

During the century that followed several ingenious writers, now for the most part forgotten, endeavoured to combine both conceptions into a systematic theory which would explain the workings of the mind in purely mechanistic terms. The most remarkable of these attempts was perhaps that of Dr. William Coward, whose notions were satirized by Archbishop King:

> A proof within himself he feels
> That all mankind is mov'd by wheels;
> Their chains and strings and hidden springs,
> And twenty other curious things—
> All first by craft of Nature made
> Ere clocks and watches form'd a trade.[1]

And then one day, towards the close of the century, Lucia Galvani drew her husband's attention to the way the frogs' legs, prepared for dinner and dangling from copper hooks against an iron grid, were twitching about as though they were alive; and, after the two Galvanis had published their novel experiments, it seemed natural to suppose that what flowed through the nerves was not 'animal spirits' but 'animal electricity'. Thereupon the science fiction of the day took yet another dramatic turn. Frankenstein, the medical student in Mary Shelley's novel, pieced together his monster out of bones and muscles stolen from the charnel house, and used the current from a voltaic pile to animate the limbs. The details (so she explains in a preface to the later edition) were suggested by "experiments ascribed to Dr. [Erasmus] Darwin" (Galton's grandfather) "and certain German physiologists". Indeed an enterprise of this nature had actually been planned by Shelley himself in his laboratory at Field Place. And from that day to this by far the most life-like hominoids have been those designed by the electrical engineer.

Looking back, we notice how, in each successive epoch, the model-maker, in his efforts to be completely up-to-date, cashes in on the latest achievement of technological gadgetry, and works it up in fact or fancy. Meanwhile, contemporary theorists echo the same theme, and argue that, if machines can thus be constructed so as to behave like human beings, then in all probability human beings are themselves constructed on machine-like principles; and today both the popular journalist who sees a sensational bit of news in the performances of the latest ' robot ' or ' electronic brain ' and the student who reads or writes about them seem equally unaware that, all down the ages, similar claims have been repeatedly made and just as repeatedly refuted. Nevertheless, it is but fair to recognize that almost every one of these ingenious devices has in its time served to illustrate some crucial feature in the behaviour of living organism; and even those investigators who are most firmly convinced that human behaviour cannot be wholly explained in mechanistic terms, can best establish their point by pushing mechanistic explanations as far as they will go, and so revealing (if they can) that there are still some residual phenomena which defy interpretation along these lines.

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[1] *Bibliotheca* (1712). Coward, who claimed John Locke's private support for his ideas, published his theory in a book called *Second Thoughts Concerning the Human Soul* (1702), where he incidentally contended that "the notion of a human soul as a spiritual immortal substance united to a human body to be a plain heathenish invention". His book was condemned by a committee of the House of Commons to be burned by the common hangman; but a second edition appeared in the following year.

" The springs and the threads that control the human puppet ", says Assézat in his translation of La Mettrie's *L'Homme machine*, " are, as we now know, all gathered up and hidden within the cerebral hemispheres ". And so the latest model-makers have become chiefly interested in ' designs for a brain '. Of modern attempts to represent psychological processes in terms of models like those described by Dr. George, the earliest, and in many ways the most instructive, were those of McDougall in his youthful behaviouristic phase.[1] McDougall began by asking, not what machines could actually do, but how psychophysical processes could be represented by systematic schemes like those described in the blueprints of machines or the circuit-diagrams of electrical apparatus. He had the advantage of an intimate knowledge of the most recent discoveries about the detailed working of the nervous system, notably those resulting from Sherrington's researches on the cerebral cortex and on reflex action; and he saw, or believed that he saw, how the simpler operations of the human mind, as analysed by the most penetrating of his contemporaries, notably Stout and the German experimentalists, could to a large extent be interpreted in terms of the Sherringtonian theory of a hierarchical system of neural circuits or ' arcs '. He supposed that the stimulation of specialized receptors started a train of impulses (' waves of electrical change '), which were then conducted along a ramifying neural network, steered in this direction or in that by the varying conditions obtaining at the nerve-junctions, and eventually discharged through executive organs (muscles and glands), thus producing a pattern of outgoing action more or less appropriate to the pattern of incoming stimulation. Whether or not a particular nerve-cell in the circuit actually fired would depend partly on its momentary state, which determined its ' threshold for discharge ', and partly on the intensities of the incoming stimuli, which, when summed, have to reach the necessary ' threshold '. The effect of the discharge could be either excitatory or inhibitory; and between each nerve-cell and the next there was a ' junction ' which allowed the current to pass in one direction only. At the same time (so he believed) it presents a varying resistance to its passage thus deciding the route along which it flows. The degree of resistance at each cell-junction was assumed to depend partly on the conditions in the field of neurones functionally adjacent to it, and partly on its recent activity (which might according to its nature leave behind a state either of facilitation or of fatigue), but most of all on its past activity, which would tend to lower the resistance permanently, and in that way set up the connections or ' associations ' responsible for memory, habit-formation, and other forms of learning. The importance of what today would be called ' feed-back ' and ' reverberating circuits ' he fully recognized; both are clearly illustrated in his diagrams. The homeostatic principle, which later inspired Ross Ashby's ' design for a brain ', was likewise frankly accepted. Stout had in fact stated it in a passage which McDougall quotes: " The teleological aspect [of attention and other cognitive processes] has its counterpart in the tendency of neural systems towards *virtual equilibrium*—a tendency that exists in every material system as such ".[1] The nervous system as a whole is depicted as a network of nerve-circuits arranged in a series of five main levels—reflex, sensori-motor, perceptual, associative, and relational. Finally, the various general principles thus outlined are applied in detail to explain the working of cognitive, affective, and conative processes at each successive level or stage; and many of McDougall's circuit diagrams, drawn up to illustrate these applications, display a remarkable resemblance to those printed by Dr. George.

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[1] Cf. W. McDougall, *Physiological Psychology* (1905), and ' The Physiological Factors of the Attention Process ', *Mind*, N.S., IX, 1902, pp. 109–132, 216–229, 324–340. McDougall's lectures which I myself attended, were freely illustrated by working models in hardware, which he and his laboratory assistant had invented. They are evidently unknown to Dr. George.

However, most of our present-day model-makers appear to start, not from a detailed knowledge of the working of the mind or brain, but rather from the recent discoveries of what can be accomplished with a stock set of electromagnetic relays, thermionic valves, transistors, or the like, ingeniously wired: and this is usually supplemented by a variety of fruitful notions taken from information theory, communication theory, and modern symbolic logic. The underlying psychology, however, remains exceedingly naïve. When we try to pick out and piece together from incidental references a complete theory of the way Dr. George supposes the mind to work, we find ourselves left with a general conception which (except for a slight modernization or, should one say, Americanization, of the terminology) is even more primitive than the theories put forward by nineteenth century associationists like Spencer or Bain, and entirely disregards the numerous criticisms and modifications introduced by Ward and Stout, Brentano and Stern, or Koffka and Köhler—or indeed by McDougall himself as he gradually discovered what his psychoneural models could really explain and what they could not.

There are, as everyone knows, two main types of mathematical machines—analogue machines and digital machines. Dr. George's contention is that, with minor modifications, the more elaborate forms of the digital computer furnish by far the best models for describing the mental processes carried out by the brain. As its name implies, a digital computer operates on a two-fold principle: (*a*) it works, not with continuous physical quantities (like the older analogue machines), but with discrete numbers—represented by two kinds of digits only—0 (switch closed) and 1 (switch open), and (*b*) it reduces all mathematical operations to processes based on counting. The entire apparatus consists of five main parts—for input, storage, self-regulation, processing, and output respectively. It is thus a mechanism which (i) accepts general instructions in the form of coded words (expressed by digits) and specific information in the form of coded numbers, and then (ii) with the aid of a rapid short-term storage system and a slower long-term storage system, and (iii) a general control-system, (iv) manipulates the numbers according to instructions, and (v) communicates the final result which is, or should be, the solution to the problem put to it.

Dr. George, after a brief introduction outlining the plan of his book, begins his exposition with a long and instructive chapter on ' cybernetics '. The word, he says, was " first used by Norbert Wiener ". This is not strictly accurate: it was in fact coined by Ampère (*Essai sur la philosophie des sciences*, 1834). Following Wiener, George adopts it to designate " the scientific study of control and communication, not only in *inanimate*, but also in *living* systems " (his italics); and as such he believes it supplies a much-needed " conceptual framework for experimental psychology ". The chapter ends with a short account of how electrical computers carry out their tasks, and how ' finite automata ' may be constructed.

The next chapter deals with ' philosophy ', and consists partly of a statement of the philosophical standpoint Dr. George thinks the cyberneticist is bound to adopt, and partly of a review of recent theories of language and logic. He adopts as his philosophical 'basis' Feigl's classification of the chief categories of 'epistemology and methodology'. This recognizes four types of 'realism' and five of 'phenomenalism': as Dr. George formulates it, however, it leaves no place for the various types of 'idealism'. Interesting as the discussion is, its relevance is not very clear, since in the end Dr. George seems to come down in favour of both 'critical realism' and 'critical phenomenalism', adding that "the two differ very little"—no doubt because, as he interprets them, the former takes the shape of a literal materialism and the latter of a hypothetical materialism. Psychophysical dualism ('mentomaterialism', as someone has inelegantly called it) is ignored, presumably on the ground that, like idealism (in the sense of 'mentalism'), it is presumed to be defunct. We are thus committed to "a strictly behaviouristic standpoint", for cybernetics (we are told) is itself "a direct off-shoot from it".

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[1] W. R. Ashby's ' Design for a Brain ' (*Electronic Engineering*, XX, 1948, pp. 35f., later expanded into a book with the same title) contains the earliest description of an electrical brain-model. It is an ' analogue machine ' consisting of a set of magnetic needles which, when deflected, return as a result of ' negative feedback ' to a central position. Stout's conception (*Analytic Psychology*, I, pp. 199f. II, pp. 287f.) was suggested, he tells us, by R. Avenarius's theory of a ' vital series ' (*Kritik der reinen Erfahrung*, 1888).

The discussion of 'logical processes' is continued in a chapter on 'Logical Nets', a concept which plays a crucial role in Dr. George's schemes. Boole, it will be remembered, in his *Laws of Thought* (1847) sought to treat logic as a kind of algebraic computation, using 0 for negation and 1 for affirmation. Boole's ideas formed the starting point of the calculus of propositions set forth in Russell and Whitehead's *Principia Mathematica*; and this in turn was adopted by McCulloch and Pitts as a basis for their system of 'neuronic nets', which were assumed to operate like a system of 'logical nets'. Dr. George gives a series of diagrams of increasing complexity, showing how (in the view of the two Chicago psychologists) the various logical processes could be simulated by a suitable arrangement of valves and wires.

The theoretical part of the book then ends with a chapter on 'programming computers to learn'. Here, however, the reader is held up by a type of difficulty which recurs again and again in Dr. George's exposition. He seldom gives formal definitions for his key-terms; often they seem to change their meaning from one context to another. At first the term 'learning', when applied to the computer, is apparently intended to mean the relatively simple process of relegating an item of information to some kind of store in which it can be retained until it is required. But presently we drift into a long description of how computers 'learn' games like chess, noughts-and-crosses, and draughts; and here plainly the word takes on a very different meaning. The summary, which ends the chapter, tells us that the argument has demonstrated how, "contrary to many opinions, computers can be shown to *learn or behave inductively*". But the two phrases are by no means synonymous. With human beings the activities so named are quite distinct; and with the machines (as indeed the inventors themselves have pointed out) the analogous operations differ just as widely. The simpler process, we are told—that of making and storing connections—"results from the initial structure of the machine; the more complex types of operation require specific programming—different for each different type".

The second half of the book sets out to explain the working of the brain and nervous system in terms of the foregoing principles, and so to demonstrate "that there is a perfectly good sense in which we can say 'the brain is a computer'". The order of the topics is a little confusing. We start with a chapter reviewing the chief 'psychological theories of learning'—special attention being devoted to those of Hull and Tolman; it is argued that theories such as these lend themselves readily to interpretation in terms of 'logical net models'. This is followed by a more general chapter on 'behaviour and the nervous system', ending with a technical description of the anatomical structure of the nervous system. Most of this, however, seems scarcely necessary: what has the location of the 'periamygdoid cortex' or the composition of the 'juxtallocortex' to do with 'the brain as a computer'?

Rather more relevant is the next chapter on 'theories of the nervous system'—those of Pavlov, Konorski, and Hebb being described in some detail. Hebb's theory is rejected as being "too versatile as it stands"; his system, it is said, "learns much too quickly to be a realistic model ". The discussion ends with a short account of the particular ' interpretation ', which Dr. George believes to be ' implied by [his] own investigations '. Human behaviour, he maintains, must depend on the working of three ' systems '—(i) a ' C-system ', which is variously termed a ' control-system ' or a ' cognitive system ', and includes a ' permanent memory-store '; (ii) an ' M-system ', i.e., a ' motivation system ', concerned, we are told, with " the designation of ' values ' to certain inputs "; and (iii) an ' E-system ': this, we are told, is an " emotional system, being a signal of different degrees of satisfaction the organism is experiencing ". " Broadly speaking, our C–system is to be identified with the cerebral cortex and basal ganglia; the M–system is surely to be identified with the effects of the internal organism, making use of the reticular system, on the thalamus and hypothalamus; the E–system is identified with the hypothalamus and the autonomic system ". The operations of all of them, it is held, can be explained in terms of " logical nets ". The explanation, however, seems incomplete and often obscure. For more detailed hints we have continually to turn to other chapters, or to Dr. George's earlier papers.

The activities of the ' cognitive system ' are discussed in greatest detail, and interpreted along the lines adopted by the old sensationist and associationist schools. True to the behaviourist tradition, ' learning ' is treated as the basic activity of all cognitive operations: even " thinking ", says Dr. George, " is in no essential way different from learning ". Learning is now described as a process of ' selective association ', i.e., of simple conditioning. The ' building bricks ' of the various systems are the same throughout. They consist of (i) " elements—to be interpreted as neurons ", which are connected (ii) by " wires or fibres of two sorts—input and output ". There are two kinds of input—excitatory and inhibitory. Each element of neuron has a threshold which is measured by the number of excitatory inputs needed to fire it: if inhibitory inputs arrive at the same time, then the neuron only fires if the excess of the number of excitatory inputs over the number of inhibitory inputs reaches the threshold value. In this way ' information ' can be transmitted from input to output by the passage of a current along the fibres from one element to another, the ' code ' being of the same " pulse/no-pulse kind which is used by computers "—a morse code with dots only.

So far, it is a scheme which nearly every neurologist would accept—" a scheme ", in Sherrington's words, " of lines and nodal points, gathered together at one end in a great ravelled knot, the brain. Its conduction paths are more than a million fold; its activity patterned, not indifferently diffuse ". " Science," he adds, " inclines to look on nature somewhat as a feat of engineering . . . And the question is, whether or how to account for human behaviour by an arrangement disposed after the manner of the switchboard of an automatic exchange ". Such a conception, he points out, would imply that " a particular configuration of paths would compel a particular result ". What we find, however, is " an immense variability ". Rather, he concludes (with an evident allusion to Goethe's famous simile), it is " an enchanted loom where millions of flashing shuttles weave a dissolving pattern—always a meaningful pattern, never an abiding pattern ".[1]

Sherrington's model, however, includes one all important feature which Dr. George's model omits—the synapses or cell-junctions. Of all the structures that make up the neural network these are the most variable and probably the most influential. It is their variability that mainly accounts for the " immense variability in the results ". In Dr. George's scheme the ' threshold ' of each neural element is given a fixed and unalterable value; long-distance memories are assigned to a separate section—the storage-system. In Sherrington's scheme they are the effects of the progressive and permanent lowering of the synaptic resistances which results from the repeated passage of the impulse. Dr. Mackay has sought to indicate how a machine might effect this ' valuational feedback on thresholds ', as he calls it; but his devices are quite unlike those of the nervous system. Dr. George's mechanism for associative learning requires a curiously complex subsystem of nerve-cells—two ' input elements ' (A and B), three ' inner elements ' (C, D, and E), and three ' output elements ' (F, G, and H). If A and B fire simultaneously, then together they excite a further excitatory element C, but do not fire an inhibitory element D to which they are also linked; C then fires E which in turn fires first H and then G; at the same time, however, in virtue of a built-in ' loopline ', E re-excites itself and so *keeps on firing* until it is inhibited. Thus, when on the second occasion A fires alone, it is reinforced by E (instead of B), and consequently still fires the output element G, although B remains unstimulated. Had A fired alone at the very outset, it would have fired F instead of G, and would simultaneously have fired D which inhibits E. Why it does not fire D, when B is also operative, is not made clear. Indeed the introduction of D seems a needless complication. Its only use apparently is to ' erase the memory ' at a later stage.

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[1] *Man on His Nature*, ch. VII, ' The Brain and its Work '.

Now, as I have suggested elsewhere, a circular process, whereby a pattern of nervous impulses is for a while " kept circling round in a closed self-re-exciting chain ", could readily account for the way in which we " keep alive a *short-distance* memory ", but of itself " it can hardly account for more permanent storage ".[1] Dr. George tells us that his notion of a re-entrant loop-line was suggested by the device employed in Dr. Grey Walter's well known ' Conditioned Reflex Analogue ' (a machine which has a built-in reflex to come to a flash of light and which can be taught by association to come when called by a whistle). I suspect therefore that the real reason for these somewhat intricate memory-nets is that this was the only way in which this type of electronic model could be made to form associations.

The description of the ' logical nets ' that make up the ' motivation system ' is far more sketchy, and much less convincing. And the reduction of the ' emotional system ' to a similar set of ' logical nets ' is affirmed rather than explained: " an E-system ", we are told, " could easily be constructed in a manner similar to the M-system, so there is no purpose in drawing up a network for it ". Elsewhere, however, Dr. George candidly acknowledges that " machines in general do not have either motivational or emotional systems "; and I find it extremely difficult to see how the " satisfaction that the organism experiences " when its " purposes are fulfilled " can possibly be reduced to purely logical and mechanistic categories.

The book ends with two chapters on perception, which is regarded as primarily a matter of classification, and thus readily reducible to terms of ' logical nets '. In order to arrive at an appropriate classification of any incoming pattern of stimuli, the neuronic model has to maintain a continual process of ' scanning ' (analogous to that by which a television picture is produced). This indeed, as Grey-Walter has observed, is " the only conclusion that would seem possible to a communications engineer "; but it is not a conclusion suggested by neurology or psychology (save in certain rather specialized cases). The ' rest of the cognitive faculties '—in particular the various processes of problem-solving, which after all form the supreme accomplishment of modern computers, and the processes of ' constructive imagination ', which is not an activity of any type of computer—are disposed of in a few concluding sentences. " This statement ", he admits, " is quite inadequate to account for the ramifications that occur under the names of ' creative thinking ', etc., but it says enough to show how such problems may be quite easily fitted into the scheme of the finite automaton ": I doubt if the reader will agree. The whole book then terminates with a ' Summary of all that has been discussed '.

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[1] This *Journal*, XIV, p. 68. I hold, however, that such circular activities are more effective if they include in the circuit a motor (or incipient motor) reaction—as indeed McDougall has pointed out. In the course of a conversation (let us say) someone gives me two telephone numbers, —7433, 2969. While hunting for pencil and paper, I keep saying them silently to myself: if, before I have written them down, my informant distracts my attention with some further inquiry, my recollection of the numbers at once disappears. The diversion inhibits the reverberative activity. But I should hold with McDougall that the inhibitory effect is the result of a ' drainage of the primary impulse ' rather than with Dr. George that it is due to a positive inhibitory impulse. The theory, as I have outlined it, seems to receive experimental confirmation from some investigators reported by Dr. Broadbent in the current issue of the *New Scientist* (' Short-term Memory ' XVI, pp. 20–21).

How far has Dr. George succeeded in proving the conclusions set forth in the argument at the beginning of his book? To what extent has he demonstrated that the brain acts 'as a computer' and that human behaviour can be interpreted in terms of the ' logical nets '? The reader, I fancy, may be tempted to reply that Dr. George's own behaviour in 'analysing and communicating information' is by no means so logical or so systematic as that of the 'nets' he postulates. If (as I fear) I have failed to do full justice to his views, it is because, in common with several other commentators, I have found his line of reasoning extremely hard to follow. Like most of our contemporary automatists (if I may call them that), he offers little concrete evidence to show that the brain and nervous system are in fact constructed in the manner assumed, or do indeed operate like the models which have been constructed. The advocates of these mechanistic theories make their views sound plausible by the metaphorical use of psychological terms in describing the working of the inanimate machines. The models are said to ' perceive ', ' discriminate ', and even to ' see ', to ' be motivated ', to ' have insight ', to ' think ', and to ' decide '. But naturally, if we start by talking of the computer as a brain, we shall have little difficulty in regarding the brain as a computer. Plainly, however, such anthropomorphic language virtually assumes the very thing to be proved.

I suggest therefore that what is needed is a preliminary specification of the ways in which the models proposed are to be considered analogous to a living nervous system, first in regard to their structure, secondly in regard to their operation, and thirdly in regard to the effects they produce.

(i). As regards *structure*, we have already noted several formal resemblances. Both models and brains are built of elements which are able to discharge energy, and which are linked by channels that conduct it; in both cases these branching and converging chains of conductors are so constructed or arranged as to form a more or less systematic network. From this it is tempting to infer that, as with the machine, so with the organism: its behaviour must depend primarily on its built-in structure—on its genetic constitution in fact—and only secondarily on the external stimuli that excite it and the memories that it has managed to store. However, as we shall see in a moment, the inference depends largely on the fact that we have been thinking chiefly of determinate types of machines that have been elaborately pre-wired at the start; in the brain most of the connections between the several elements are acquired during its life-time—a difference that appears to result from the fact that the mechanistic models rarely contain anything analogous to the variable synapses, which constitute such a distinctive feature of the brain.

But there are other differences, still more significant, between the two types of structure. First of all, the number of unit-elements in the larger computers is only of the order of 10^4; the number of nerve-cells in the brain is of the order of 10^{13}, all housed in a skull that is barely 18 inches in circumference. Secondly, the working parts of which the computer is constructed are usually composed of simple inorganic substances, largely metallic, so as to conduct an electric current; the nervous system is composed of highly complex organic substances, mostly colloidal, which have specialized properties of their own—in particular those elusive physico-chemical properties that we term life. In short, the elements in the model are dead; the cells of the nervous system are very much alive.

(ii). In regard to *mode of operation*, the differences are still more striking.

(*a*) Consider first what we know of the way in which the *computer* works. Like the nerve-fibre, the computer-circuit operates by means of the trains of pulses which it carries. But the use made of the pulses is quite different. The digital computer, as we have seen, proceeds by counting the pulses: what it does, therefore, depends on an unvarying and specific response elicited by each pulse as such. In the nervous system, however, the number of pulses required to excite one and the same nerve-cell is not a fixed quantity: it fluctuates widely from time to time; hence there is little that is unvarying or specific about the consequences that ensue, and rarely anything that corresponds to actual counting. The operations of the digital computer are essentially analytic: indeed they are usually serialized. On the other hand, the nervous system operates synthetically—or, as we have been taught to say 'integratively'. The human eye, for instance, can take in, as a Gestalt-like whole, a vast and complicated scene; the 'finite automaton', like the television camera, has first to translate its 'input' into a long stream of signals. And generally speaking whereas the computer commonly works by converting spatial patterns into temporal, the brain usually works by converting temporal patterns into spatial.[1]

These and other fairly obvious differences might, I fancy, be summed up by saying that the only types of problem that a computer can solve are those which, in the broadest sense, have computable solutions. There is a well known distinction between two types of problem, and two corresponding types of thinking, with which all who construct intelligence tests have long been familiar. Most of the questions included in the earliest test-scales were 'open questions'; for example, in the Binet–Simon Scale (first British version) —"Suppose you had to undertake something important: what should you do first of all?" or "One day a boy, walking along the road, picked up something shiny, and, running to the nearest policeman, said: 'I've just found a . . .'. What do you think it was?" Such questions can only be answered by what I have termed 'inventive thinking'. But a later version substitutes for the latter question: "Every boy has two parents, four grandparents, and so on: how many great-great-grandparents has he got?" The advantages of this second type of problem (and, what is too often forgotten, its disadvantages) arise from the fact that it permits of only one correct answer, and that can be found quite mechanically by applying the appropriate rule. This I have called 'applicative (or methodical) thinking'.[2]

The distinction has a long history. In logic it has cropped up again and again in discussions of 'induction' from Aristotle to the celebrated controversy between Whewell and Mill. In mathematics, and still more recently in information theory,[3] it has become customary to distinguish between problems that can be solved by an 'algorithm' and those that can be solved (if at all) solely by 'heuristic' methods. The computer can solve only problems of the former type—'closed' questions which can be answered by the application of a rule, formula, or algorithm. Human beings, however, perhaps because their brains work so slowly, tend usually to solve their problems heuristically, even when the solution *could* be reached by a methodical technique. Imagine a computer answering this morning's crossword clue which asks for "A word of 18-letters meaning a 'doctrine'": it would presumably try fitting into the 18 places each of the 26 letters of the English alphabet in turn; out of the combinations so obtained it would then reject those that were unpronounceable, and systematically compare the rest with all the meaningful 18-letter words in the Oxford Dictionary: or it might start with the dictionary and work through that. In either case I may leave the reader to estimate the time required. The slow-working brain will hit on the answer in a minute or two. There are, of course, many tasks in which the computer can unquestionably beat the human mind. It can solve complicated equations in an hour or two which a team of human calculators could not solve in a life-time. Ask it to find the value of π to more decimal places than any human mathematician has hitherto obtained; and it could perform the job in less than an hour: but it could never tackle the task of (say) finding a five-act play about Hamlet better than any human dramatist has hitherto written. It is this grand contrast in their mode of operation that accounts for those unique qualities of human thinking which distinguish it from the performances of the mere machines—qualities which we commonly describe by such words as intuitive, creative, versatile, and spontaneous.[1]

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[1] For the processes whereby it is, or could be, effected, see J. T. Allanson, 'Some Properties of a Randomly Connected Neural Network', *Information Theory: Third London Symposium*, 1955, pp. 303–313 and refs.

[2] Cf. D. Wheeler, 'Studies in the Development of Reasoning in School Children', this *Journal*, XI, 1958, pp. 154f. and refs.; C. Burt, 'The Structure of the Mind', *Brit. J. Educ. Psychol.*, XIX, 1949, pp. 190f. and refs. Guilford's distinction between 'convergent' and 'divergent' thinking refers to much the same broad differences.

[3] A. Newell, J. C. Shaw, and H. A. Simon, 'Elements of a Theory of Human Problem Solving', *Psychol. Rev.*, LXV, 1958, pp. 151–166.

(*b*) Let us now briefly consider what little we know about the mode in which the *nervous system* operates. Since from their very nature they are activities of a biochemical organism, the processes occurring within the nerve-cell, the nerve-fibre, and the nerve-junction, are essentially chemical processes; the processes occurring within the digital computer are essentially electrical. Hence for one thing the extraordinary contrast in speed between the two types of operation: present-day computers, for example, can perform a multiplication at something like five million times the speed of a practised mathematician. It is this that enables the electronic computer to compensate for the circuitous and repetitive procedures which it automatically adopts. Unlike the pre-wired circuits of Dr. George's models, what the nerve fibre conducts is not an electric current which rushes along with a speed approaching that of light; it is a complex cycle of electro-chemical changes in and around the film-like skin of the nerve-fibre, propagated at much slower rates (roughly 1 to 100 metres per sec according to the thickness of the fibre). Even the methods of storage seem to be chemical rather than physical—chemical changes at the synapses or perhaps, as Dr. Hydén and others have suggested, changes effected by rearranging the sequence of the nitrogenous bases which form part of the RNA of the cells. Nor are we concerned with isolated impulses running along single and specific paths to sharply localized centres, but rather with waves and patterns of energy, often using much the same conducting-paths for very different purposes.

The inevitable consequence of all this is that the only adequate description of the detailed working of the brain must be statistical. It is for this reason, I take it, that mental operations have lent themselves so readily to statistical study by means of ' factor analysis ', with its accessory devices of matrix formulation and the calculation of probabilities. Moreover, in my view the changes that take place at the cell-junctions must be of that minute order which require interpretation in terms of principles and concepts, drawn from quantum theory, notably the ' uncertainty principle '; and this, too, entails an approach along statistical lines, and is bound to defeat any attempt to formulate the facts and the inferences in terms of mechanistic or deterministic concepts, or, as Dr. George aspires to do, " in terms of mathematical precision ".

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[1] The rules which the machine applies may of course include statistical rules, i.e., those which yield not certain but only the most probable solutions. They may, too, include rules for ' trial and error '. For this reason the term ' heuristic ' in a psychological context may be a little confusing. I suspect that it is ambiguities of this kind that have led Dr. George to maintain that problems that " occur under the names of ' imagination ', ' creative thinking ', etc." may be " quite easily fitted into the scheme of the finite automaton ". He refers, for instance, to the way computers have been programmed to ' learn ' how to play a game like chess. But, if we examine the actual procedure, we quickly discover that the operations carried out by the machine are quite unlike those of a skilful chess-player. Imagine (*a*) a computer and (*b*) the youthful Morphy being supplied for the first time with the basic instructions about the rules of the game. Each then sets out to ascertain, by examining the relevant consequences, which of the possible 28 first moves White should select. Let the reader consider how each would go to work, and how many ' mental trials ' each would

(iii). Finally, we have to consider how far the overt *effects* produced by the activities of the artificial model resemble or can represent the effects produced by the activities of the human brain or mind. It is chiefly on this aspect of the problem that the automatists have hitherto concentrated their attention. We may willingly agree that their models are capable of simulating, to a limited degree, those simpler types of human behaviour that are described as discriminating and remembering; they are, too, capable of a certain degree of self-regulation. But the resemblances extend *only* to a few of the most elementary forms, and, when all is said, seem manifestly superficial. The ' purposes ' with which the models are endowed have to be inserted in advance by the manufacturer or the programmer. But of all the effects produced by, or associated with, the human mind the most characteristic is consciousness. No one has yet claimed that he knows how to construct a working replica which would be genuinely conscious. It has required a conscious human mind to invent and build the models; it is hard to imagine a model spontaneously undertaking to invent and build a conscious human organism.

Dr. George seems vaguely aware of these various objections. But his method of meeting them is far from convincing. In his closing chapter he refers to " attempts to understand consciousness from a behaviouristic and therefore from a cybernetic point of view "; but, he adds, the problem " really has little to do with cybernetics ", and is excluded by " our behaviouristic commitment ". But this evades the issue. All the available evidence indicates that consciousness can *not* be fully understood from a " behaviouristic point of view ", which, as Dr. George interprets it, is a purely materialistic point of view. The stock criticisms brought forward by the psychologists of the introspective school will doubtless be dismissed as both biased and out of date. So let us turn once again for evidence to our leading neurologist. In the book already quoted (*Man on his Nature*, pp. 271f.) Sherrington enumerates a series of physiological, surgical, and experimental observations (including *ad hoc* experiments of his own which he regards as crucial) all of which reveal, so he argues, clear-cut instances of ' integration in consciousness ' without any corresponding ' integrative action in the nervous system '. In many cases, even the *mode* of integration, he says, differs from anything that could be attributed to mere neural integration in a material brain. His conclusion is that the underlying integrative action " is not a physiological conjunction in physical space, but a temporal conjunction in ' mental ' space ". For an equally unprejudiced opinion we can appeal to the professional engineer. Those of them who have considered the psychological aspects of the problem appear almost unanimous in rejecting a purely mechanistic theory of mind. And in this they are speaking with a specialist's knowledge of what mechanisms can and cannot do. To the layman—and in this field almost every psychologist is a layman—the recent achievements of electronic engineering, the Sputniks and the Telstars, the Ermas, the Eniacs, and the Leo Mark IIIs, are startling novelties that arouse both awe and admiration. To the engineer they are as natural and as comprehensible as the works of a wrist-watch to a schoolboy who has already

make before deciding that in the long run either P to K4 or P to Q4 would probably be the best. Automatists love to remind us that " the more imaginative types of finite automaton " have " even written poems and worked out musical compositions ". But all that the computer can really manage is, after being furnished with the categorized ' information ' about the rules of syntax, metre, and rhyming in the one case and of melodic and harmonic sequences in the other, to throw together words or notes, more or less at random, within the limitations which those rules prescribe: the human programmer then picks out and plays to us what he considers the best of his machine's many efforts. What no computer will ever do is to start suddenly breaking the orthodox rules in ways that are at once effective and satisfying—as Shakespeare and Victor Hugo did in poetry, and Beethoven and Wagner in music. We can imagine a ' finite automaton ', programmed in 1912 (let us say), managing to find ' a Lost Chord ', but not composing even the first few bars of *Pierrot Lunaire* or *Le Sacre du Printemps.*

taken the kitchen clock to pieces. What so strongly impresses the engineer is not the way the brain resembles the computer, but its "overwhelming superiority".[1]

But, after all, why should there not be something like a cybernetics of mental processes as well as of mechanical and electrical? All that would be necessary, I take it, would be to add a new dimension or set of dimensions to the explanatory 'space' which our symbolic diagrams and calculus imply.[2] Since the most conspicuous feature of consciousness is its continuous character, and its most obvious function is to represent the world by analogy, the appropriate type of mechanism for imitating its processes would seem to be, not the discrete digital computer, but the 'continuous operator' (or, as the Americans have taught us to call it, the 'analogue machine')—a type which was first in the field, but has been unduly neglected by psychological automatists.

Dr. George's treatment of the statistical aspects of his subject is brief and disappointing. In the main his interests plainly lie with the determinate types of machine. Such a machine can of course readily be converted into an inderterminate type by the simple expedient of interpolating a randomizer in one of the circuits. The output then necessarily becomes uncertain, and can only be predicted with a specifiable degree of probability. A 'stimulus response net' of this kind 'with random element' is the only diagram given by Dr. George to illustrate the working of 'probabilistic machines'. But plainly this is not the way in which the indeterminate behaviour of human beings is produced. In my view, by far the best discussion of the problems involved was that contained in a series of lectures by the late D. A. Sholl, Reader in Anatomy at University College, which I hope may still be published. As his book on *The Organization of the Cerebral Cortex* amply testifies, he was a brilliant histologist, an ingenious experimentalist, and a first-rate mathematician; readers acquainted with his contributions to this journal will realize that he was equally familiar with the methods and results of both factor analysis and cybernetics. The models he proposed were inspired by "the principles that emerge from the quantitative findings of recent histological and neurological research, and based on probability theory ".[1] They make full use of matrices and matrix algebra, and thus largely resemble the procedures of the psychological factorist.[2] However, neither Sholl's model nor his arguments are discussed by Dr. George.

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[1] Cf. Professor R. O. Kapp, *Mind, Life, and Body* (1951) and refs.: A. Tustin, 'Do Modern Mechanisms help us to understand Mind', *Brit. J. Psychol.*, XLIV, 1953, pp. 25f. "The conclusion must be" (says Dr. Tustin, and he too is speaking as a highly skilled electrical engineer) "that the detailed processes specially characteristic of digital computers are *not* likely to be enlightening in respect of the work of the brain". Of the two obvious alternatives, he himself believes that "electrical engineers may perhaps be disposed to the latter, namely, that the phenomena of consciousness constitute a distinct field which interacts in comparatively rare situations with material phenomena". And this conscious or mental field, he suggests, may be as different from the fields at present known to physics—and perhaps as unexpected—as electrical fields were before the discoveries of Faraday and Maxwell. *If* therefore we are to think of the mind as a kind of mechanism, then "our conception of mechanism must be extended". For a lucid statement of the automatist's arguments see D. M. MacKay, 'Mindlike Behaviour in Artefacts', *Brit. J. Phil. Sci.*, II, 1951, pp. 110f. and 'Mentality in Machines', *Arist. Soc. Supp.*, XXII, 1952, pp. 61f.

[2] This mode of approach was briefly touched upon in one or two earlier papers in this *Journal* (e.g., XIV, pp. 53f. 'Factor Analysis and its Neurological Basis' and refs'; cf. XII, p. 86, footnote 1 and refs.). The suggestions there made have evoked inquiries from several correspondents. Here I need only state that what I had in view was something like the 'psychophysical cosmology' set out in Eddington's posthumous *Fundamental Theory* (1946: for a more recent attempt see C. C. L. Gregory and A. Kohsen, *The O-Structure*). This mode of bringing together "the two incommensurable but complemental and concurrent components of one serial event" (physical energy and conscious mind) by allotting them separate but intersecting spaces has been suggested, not only by physicists and philosophers, but also by more than one neurologist. "A mirror" (says Sherrington) "contemplating itself and its reflection of the world, might say: 'I am 2-dimensional—a surface only; my ways belong to 'Flatland''". And yet it might guess that it is in fact "immersed in a world that has depth"; and so eventually "divide the world of its experience into two categories—this 2-dimensional, that 3-dimensional, somewhat after the paradigm of our two categories" (namely, the familiar three-dimensional space of the physical world and the further

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dimensions of the mental world): *op. cit.*, pp. 349f. Cf. also J. R. Smythies, 'A Note on Physical and Psychological Space', this *Journal*, XIV, pp. 141f. and refs.

The analogy is by no means new. Sherrington's mention of 'Flatland' is an allusion to an early work of scientific fiction with that title, first published in 1884, by a writer who signed himself 'A. Square'; he was in fact the Rev. R. A. Abbott, Headmaster of the City of London School, whose *English Lessons for English People* was a stock textbook for schoolboys like myself. The inhabitants of 'Flatland' consist of triangles (workmen), squares (humbler professional classes), hexagons (upper professionals), and circles (government officials). The parable culminates in a dream in which a Square visits 'Lineland', and tries to persuade the King that the universe contains more dimensions than one; then he himself receives a visitor from three-dimensional 'Spaceland', who appears and disappears like a ghost as he passes through the two-dimensional Flatland (both Fechner and Zöllner had invoked a fourth spatial dimension for the same purpose). The converted Square thereupon preaches the gospel of a multi-dimensional universe, and the story ends with his arrest and condemnation. One of the merits of the conception, so it was claimed, was the interpretation it suggested for many of the phenomena which the Psychical Research Society was seeking to investigate. A more ambitious novel was C. H. Hinton's *Episode of Flatland* (1907). Hinton was the son of a famous London surgeon (author of several philosophical essays and a friend of George Eliot). Young Hinton married the daughter of Boole, the logician and mathematician; after emigrating to America, he became professor of mathematics in Princeton University, and there published more serious contributions on *The Fourth Dimension* and *A New Era of Thought*. Of late five-dimensional field theories have been freely proposed by physicists in relativity, in quantum mechanics, and to explain continuous creation. If therefore we follow Dr. George and construct our machines on paper, we can also allow ourselves—on paper—a five-dimensional space. Most of these conceptions, however—like those of factor analysis which is also multi-dimensional—treat the problem statically rather than dynamically (or 'cybernetically', as Dr. George would say). So here is an opportunity for further research. Those who wish to learn about the latest speculations along these lines will no doubt find them in H. A. C. Dobbs' forthcoming Tarner lectures on 'Time in Physics and Psychology'.

[1] The earliest model of this type was briefly described in a joint paper by D. A. Sholl and A. M. Uttley (*Nature*, CLXXI, 1953, pp. 387f.). Later developments of the same general principle were published by Uttley: (see more especially 'The Probability of Neural Connexions', *Proc. Roy. Soc.*, B, CXLIV, 1955, pp. 229f.). Dr. George gives a diagram of " a simple example of a hardware realization of a classifying system constructed by Uttley " (his first machine); but George himself apparently prefers a model " constructed in terms of a theory of nets ". One has been described by him in a paper already published (*Automation Prog.*, III, 1958, pp. 19–21: see also *id. ap.*, *Models and Analogues in Biology*, XIV, 1960, pp. 169–191). MacKay seems to hold that the apparent originality of the human mind is a by-product of its statistical working: " *Originality* of a kind can always be in evidence because of the lack of determinate links between input and output " (*loc. cit. sup.*, p. 111: his italics).

[2] In an earlier chapter Dr. George breaks off his account of the 'McCulloch-Pitts Neural Net' to introduce a brief algebraic explanation of what a matrix is, and illustrates the processes of matrix addition and multiplication. He ends his explanation by telling us that any input of a finite automaton can be represented by a matrix consisting of 0's and 1's; and then abruptly turns to the subject of logical inference and the representation of primitive logical connection by cell-like elements and connecting fibres. No actual use is made of matrix algebra; and the chief reason for its adoption in psychological analysis—the reduction of matrices to a 'canonical form'—is never even mentioned. I briefly summarized the possibilities of this mode of approach in *Psychometrika* III, 1938, pp. 160f. To put it at its simplest, it assumes that we have to deal with the correspondence between two matrices, which in their simplest form may be expressed as $f(X_1, X_2, X_3, T)$ and $f(x_1, x_2, x_3, t)$, where the symbols in capitals represent coordinates in space and time in the external world, and those in small letters represent the coordinates of the processes within the brain and in what may be called personal time. The 'real object' and our conception of it are then represented by the 'canonical forms' of the matrices.

S.P.

Sholl points out that there are two prevailing types of theory in regard to the activity of the cortex: (i) ‘switch-board theories’, which for obvious reasons are the most popular, and (ii) ‘field theories’, such as those of Köhler, which are based on analogies with the field theories adopted by physicists in their electromagnetic inquiries. Interpretations of this latter type are rather summarily dismissed by Dr. George on the ground that there is “no evidence to support a field theory of neurology”. The former have been just as summarily rejected by Dr. Sholl, at least in the naïve forms favoured by the earlier model-makers. Having reviewed the histological evidence he himself concludes that “it is most unlikely that the unique, complicated, permanent circuits” (which such models presuppose) “exist in any nervous system”. But are the two theories so utterly irreconcilable as their supporters assume? What seems to be wanted is a hypothesis or a model which will embody the merits of both without the drawbacks of either. Such a compromise, I suggest, is largely achieved by the type of model outlined by Sholl and developed by those who have followed in his steps.[1]

An important improvement in the Sholl model was introduced by Beurle.[2] He points out that, whereas for simplicity, Sholl's model arranged that the information obtained from the frequencies of previous occurrences should be stored in *individual* units, the neurological counterpart of these units would be, as Sholl's own work implied, a *large aggregate* of nerve-cells, widely dispersed: (this incidentally would explain why localized damage so often fails to abolish the information so stored). In his second paper Beurle introduces the further assumption that the threshold for any nerve-cell would be slightly lowered each time it is activated, which makes his model still more realistic. Cragg and Temperley[3] in an illuminating paper (which again is not included in Dr. George's list) have suggested that a plausible analogy may be drawn between the organization of the closely packed neurones of the cortex and their activities and the ' cooperative phenomena ' which are exhibited by substances like ferromagnets, where the constituent atoms are closely packed and so tend to form a pattern of ' domains '. The properties shown by such substances are qualitatively different from those which would be inferred when there are no such interactions (e.g. when the magnet is heated) or when the interactions are ignored. Both types of phenomena lend themselves most readily to investigation by the methods of statistical mechanics.

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[1] The theory of learning and extinction implied in Uttley's later model has been criticized by Broadbent (*Perception and Communication*, 1958). Dr. George, if I understand him rightly, endorses the criticism for this particular model. But he maintains that “from what [he, George] has already said about logical nets, it is obvious that one could be constructed with the desired properties”. Few readers, I fancy, will find this ‘obvious’; nor does Dr. George give any hint as to how it could be effected. In an earlier issue of this *Journal* (IX, pp. 49–62) I tried to show how a theoretical model for the learning process might be constructed with the aid of factorial matrices: one series represents the state of the learner's mind at the outset and after each trial; another the successive stimuli; yet another the conditional probabilities. The entire process as thus described could be carried out by a suitably constructed electronic computer. But in this case, as in so many others, I can see little gain in the actual construction in hardware beyond the entertainment to be got from watching a toy machine perform its antics.

[2] R. L. Beurle, *R. R. E. Memoranda* 1042 *and* 1043, Ministry of Supply, 1954. Dr. George briefly mentions Beurle's models, but does not, I think, fully realize or accept their implications. He tells the reader that “the assumption of random connectivity is one that can be introduced into logical nets, from which starting point, by growth, or the non-use of pre-wired connexions, the specific logical nets can be derived”—not a very lucid answer. And he leaves the reader to guess *how* such nets would be “derived” or how the “*non-use* of pre-wired connexions” could produce them. Quite recently several machines still more elaborate have been developed by Pask and Chapman, based on assumptions similar to those of Beurle: see A. G. Pask, ‘The Growth Process in a Cybernetic Machine’, *ap.* National Physical Laboratory Symposium in *Mechanization of Thought Processes* (1959) and B. L. M. Chapman, ‘A Self-Organizing Classifying System’, *Cybernetica*, III, 1959, pp. 152–161.

[3] B. G. Cragg and H. N. V. Temperley, ‘The Organization of Neurones: A Cooperative Analogy’, *EEG Clin. Neurophysiol.*, VI, 1954, pp. 85–92. The ‘domain’ hypothesis (due to P. Weiss) in its original form was essentially a molecular field theory. Heisenberg has shown that in ferromagnetic materials the mutual interactions are the result of ‘exchange forces’ deducible from the quantum theory of spinning electrons.

I believe that, in the changing patterns exhibited by the visual field, when the eyes are closed and attention at a minimum, we have something like a direct experience of the results of somewhat similar interactions. In part, they lend themselves readily to an interpretation in terms of the Cragg–Temperley hypothesis; but they also demonstrate the importance of non-random factors.

The refinements contained in these various suggestions Sholl himself readily accepted. Models based on them, he holds, " are far in advance of their predecessors in their agreement with the known anatomical and physical features of cortical organization ". To my mind they carry with them two instructive corollaries: first, that the built-in arrangements of the older pre-wired models must now be regarded as entirely out of date and frequently misleading; secondly, that all future models must be based on probabilistic concepts and interpretable in terms of statistical theory.

Many of the criticisms I have raised could, I fancy, be met by a few incidental corrections when Dr. George's book is revised for a further edition. If I seem to have emphasized its defects rather than its merits, that is because I believe that, as a compendium of interesting facts and highly suggestive views, it is already so good that, with a little more time and trouble, it might have been made so much better. As it stands, the reader feels that many of the pages present, not a finished and systematic exposition, but rather the author's preliminary notes for such an exposition. In almost every chapter there are signs of hasty compilation—numerous threads never taken up or tied together, minor misprints, slips in grammar and spelling, references to figures wrongly numbered or figures without any references in the text. Nor do I think the proofs of his main conclusions either clear or cogent. Indeed the conclusions themselves are often so ambiguously worded that the reader is continually left wondering whether he has correctly grasped what the author wanted to convey.

Nevertheless, in spite of the objections I have raised, I would not for one moment wish to deny that the construction of models like those Dr. George has described has a scientific value of its own. First of all, however much a psychologist may feel convinced that, in order to explain the workings of the mind, he will be forced in the end to fall back on specifically mental concepts, he is, in my view, only justified in taking such a step as a last resort: it is his duty to push familiar and well established means of explanation to their utmost before he ventures to invoke concepts that are entirely new. And this methodological maxim can best be demonstrated and driven home by trying to reproduce, so far as possible, the chief types of human behaviour with the aid of known devices which in themselves are entirely non-human.

Secondly, one way of analysing a highly complicated activity into its essential stages and constituents is to consider how one would set about achieving the same end if one had to start from scratch. An untutored savage, standing in a moving canoe, can hit a flying waterfowl with a well-aimed arrow or stone. Dr. Tustin analyses the whole complicated causal chain—which, with a human being, involves eyes and eye-reflexes labyrinth and labyrinthine reflexes, arm-muscles with their proprioceptors, sensory and motor cortex and nerves—by comparing it stage by stage with the detailed working of the component mechanisms needed to aim a shell fired from a gun on a moving ship at an aircraft target—radar-receiver, gyroscope, predictor with its feedback, motor control, servo-positioning, and the like (*loc. cit. sup.*, pp. 25f., figs. 1 and 2). The result is an admirable demonstration of what a psychologist would call ' the representative theory of perception '; and he incidentally shows that even if the ' representation ' is achieved by spatial mechanisms, the mechanical representation of the spatial components (range, elevation, angular velocity of flight, etc.) " is not ' in space ' in the same sense ". He rightly adds that " there is of course nothing in this comparison of the mind with a predictor that is an argument for the 'mechanistic view'".

Thirdly, each of those who have constructed models like those referred to in Dr. George's book—Ashby, Shannon, Uttley, Turing, Grey Walter, and the rest—has had his own ingenious theory as to how some characteristic feature of human behaviour might be produced; and the most effective way of testing such a theory is to embody it in an actual model, and to demonstrate that the notion really will produce the effects predicted. The next step—too often omitted—is to show that other consequences, deduced from the theory and displayed by the model, are also observable in the brain. Thus several striking corollaries derived from Cragg and Temperley's concept of a cortical pattern of domains have been empirically confirmed—e.g., the time of the first appearance of certain types of electrocorticogram, and the widespread and complex nature of the disturbances that result from a single afferent impulse.[1]

Fourthly, when the model is in actual operation, it often comports itself in new and highly suggestive ways, some of which might perhaps have been predicted but were not, while others come as a complete surprise. To the layman more especially the amazing variety of behaviour displayed by such mechanisms, in spite of their comparatively simple construction, is quite startling. Most models too, as Grey Walter remarks, "show almost inexplicable changes of mood: however carefully their circuits are designed, minute differences become cumulatively amplified to generate temperaments and tempers". The unexpected performances of Cora (his 'conditioned reflex analogue') were still more instructive. We are told, for instance, that "had nothing been known about the differences between appetitive and defensive reflexes, or about the relative value of reward and punishment, it could have been deduced from the uncalculated behaviour of the model". But "the most intriguing and unsuspected of Cora's complexities" was the revelation that "three types of 'memory' are demonstrably needed". (For the details of these discoveries the reader must be referred to Grey Walter's book, *op. cit.*, pp. 127f.)

None of this, however, suffices to demonstrate that (in Dr. George's phrase) "human beings and animals are essentially very complicated machines", if by that we are to understand (as the context implies) that they are *nothing more* than machines—merely "deterministic systems which could be constructed or reconstructed in the laboratory". The biochemist frequently sets up similar models to illustrate and study, in schematic form, certain component activities of the living cell; but he never claims that he can actually "construct or reconstruct" such a cell in the same way as Wöhler and his successors succeeded in constructing urea and other complex organic compounds, 'without either man, dog, or kidney'. Such chemical syntheses have indeed only become possible from a knowledge of the structural formulae for the combination of atoms established by investigators like van't Hoff and Kekulé. With the biochemist the observations and theories come first; whereas the modern psychological automatist seems to consider first of all how he can make a model, and then fits his theories to the model he has made.

No doubt in certain important respects every organism, and every organ in the organism, functions as a machine. But it is essential to what Dr. George calls 'the mechanistic approach' that the model of the human brain or mind should work on *exclusively* mechanistic principles—mechanical, if it is a mechanical model, electronic, if it is an electronic model. And, limited though our present knowledge avowedly is, one thing stands out clearly: body, brain, and mind do *not* work *solely* on mechanistic principles of these simple types. Even if,

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[1] The latter require for their detection, not a single pair of electrodes, but more than twenty: (cf. J. C. Lilly, 'Instantaneous Relations between the Activities of Closely Spaced Zones in the Cerebral Cortex', *Amer. J. Physiol.*, CLXXVI, pp. 493–564 and J. C. Lilly and R. B. Cherry, 'Surface Movements of Figures in Spontaneous Activity of Anaesthetized Cerebral Cortex', *J. Neurophysiol.*, XVIII, pp. 18–32). As Sholl and others have pointed out, these effects would be quite unpredictable from previous theories.

per impossibile, we could demonstrate that the brain itself operated simply in the manner of a glorified computing machine, we should still have to ask—What does it compute? And why? It is often said that the mechanistic model " can from its very nature have no *purposes* except those which are inserted in it by the maker or the programmer "; it is still more important to observe that it can have no *problems* except those which its maker or programmer inserts. Cybernetics and all that it implies may indeed be (as we are repeatedly reminded) " a penetrating set of novel concepts which have not as yet been pushed as far as they deserve ". But let us also remember the warning of La Rochefoucauld's oft-quoted *maxime*: " Le plus grand défaut de la pénétration n'est pas de n'aller point jusqu' au but; c'est de la passer ".

CYRIL BURT

BOOK REVIEWS

The Foundations of Statistical Inference. By L. J. SAVAGE and others. London: Methuen, 1962. Pp. 112. 15*s*.

Some three years ago Professor Savage, who was visiting London, was invited to address the Joint Statistics Seminar of Birkbeck and Imperial Colleges. The monograph now published gives an expanded version of his lecture and five shorter contributions by various speakers, together with the general discussion that ensued—all slightly revised and rearranged. Psychologists will find it an admirable sample, concise and up-to-date, of current views on the fundamental problems of statistical inference.

Professor Savage, as those who are familiar with his textbook will be aware, holds that all probabilities should be regarded as measuring subjective degrees of belief, and that statistical inference should be systematically based on Bayes's theorem. The stock objection to this view is that the use of Bayes's theorem seems to imply either first-hand knowledge of the prior probabilities, or, failing that, some recognized postulate to take its place. Fisher, as is well known, proposed to drop the appeal to Bayes's theorem altogether and to substitute the principle of maximum likelihood, treating this as the foundation on which statistical inference should rest. Other critics have proposed other devices; and a sharp division of opinion has arisen between those who adopt Fisher's principles and those who prefer the more recent methods of Egon Pearson and Neyman.

Of late there has been a growing tendency—particularly among statisticians who are unwilling to commit themselves wholly or exclusively to a frequency-theory of probability—to regard the criticisms urged by the various frequency-schools as somewhat too sweeping. Savage himself contends that most of the difficulties can readily be overcome by frankly accepting the concept of 'subjective probability' instead of limiting statistical inference to the narrower types of argument to which the frequency theory would restrict us. He regards Dr. I. J. Good's book[1] as " a landmark in its statistical reawakening ". Good uses the notation $B(E/H : M)$ to denote " the degree of belief in E if H is assumed and the believer's state of mind is M". The explicit dependence on M renders the degree of belief subjective. Keynes and Jeffreys assume that there is a 'reasonable' degree of belief which is independent of M, and in that sense objective rather than subjective. In any case beliefs may undoubtedly be compared in regard to strength or intensity even when the 'degrees' cannot be measured numerically. Accordingly, if we assume that the particular beliefs to be discussed are reasonable unless and until they have been proved contradictory, we may omit M and rewrite the formula $P(E/H)$. Good maintains that this approach will enable us to deal with both the subjective and the objective aspects of the problem within the framework of a single aspect—as indeed Carnap, Nagel, Koopman, and Bartlett would prefer to do.

De Finetti, who is commonly regarded as the most subjective of contemporary theorists, has independently adopted a somewhat similar approach, and has endeavoured to give it a still more stringent formulation.[2] His exposition appears to have exercised considerable influence on the later development of Savage's views. Savage himself, however, suggests that subjective probability can to some extent be rendered objective by expressing it in terms of the odds which one would be prepared to bet. And his exposition is here chiefly concerned with showing how this standpoint helps us to overcome many of the perennial problems that crop up in all discussions on statistical inference.

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[1] *Probability and the Weighing of Evidence* (1950).

[2] 'Sull' impostazione assiomatica del calcolo della probabilità', *Annali Triestini* (1949) Ser. ii, xix, pp. 29–81: cf. his briefer paper on 'The Foundation of Probability', in *Philosophy in the Mid-Century* (Florence, 1958), pp. 140–148.

Such an approach, as the discussion plainly demonstrates, tends to evoke strong criticism from those who dislike anything that savours of subjectivity. If, as Savage proposes, the investigator is to reach the prior probabilities by "asking himself how he would bet", this, it is said, implies that he must "perform acts of introspection rather than collect records". But, as Savage points out, the investigator's betting would be largely based on records if they exist or can be collected.

Much of the divergence of opinion, however, seems to arise from differences in the definitions, explicit or implied, for the basic terms and concepts that enter into statistical discussions. What, for instance, do we understand by statistical data and statistical inference? Many of the disputants, but not all, seem to subscribe to Khinchin's well known dictum: "the *individual* case is devoid of interest"; "statistical data", says Bartlett, "are data about classes or groups of individuals rather than about the individuals themselves". This would rule out such questions as "what are the chances that Johnny will pass the eleven plus?"—a type of question that Professor Savage would clearly admit. Practically all the participants seem agreed as to the general nature of statistical inference, namely, that it is (as Professor Barnard puts it) "that part of scientific inference in which quantitative measures of uncertainty are employed". Professor Bartlett prefers a still more objective phraseology, and describes it as "the mathematical theory which relates to observational data arising from a *physical background of chance*". Several speakers, however, have suggested that confusion might be avoided if nouns were used to differentiate the various concepts rather than adjectives—e.g., if instead of contrasting 'subjective' probability with 'objective' or 'physical' probability (whatever that may mean), we termed the former 'credibility', 'plausibility', or 'acceptability'.

Most psychologists, I fancy, would be prepared to endorse Professor Savage's main contention, namely, that most rational men not only assume that the world behaves as if there were such things as physical probabilities, but also, alike in their behaviour and in their arguments, rely largely on so-called subjective probabilities as well. Hence we can at least agree that a more systematic investigation of these 'subjective probabilities', side by side with the more familiar 'objective probabilities', will be well worth while. As to the deeper issues which evidently possess the strongest attraction for the mathematical statisticians, the reader will not be astonished to find that, even at the end of the whole discussion, no final consensus was arrived at. To my mind indeed the chief value of the book is that it brings out so clearly what are the main points of dispute between the theorists of the present day.

CYRIL BURT

Statistics in Psychological Research. By WILLIAM S. RAY. New York: The Macmillan Company, 1962. Pp. xiv+303. 45*s*.

The course outlined in this book was developed while the author was teaching students of psychology, education, and economics at Pennsylvania State University, where he was Associate Professor. It was intended as a one-semester course, and is therefore somewhat limited in range. Considerable space is given to elaborating the idea of psychological statistics as part of the 'language' of the research psychologist; and an attempt is made to "organize the whole discussion of statistical issues around certain notions about the study of language"—namely, those developed by Carnap and Morris. The opening chapter is devoted to a brief explanation of their views, together with a discussion of 'syntactics', 'semantics', and 'pragmatics' as three levels of comprehensiveness in language analyses". The headings of the chapters that follow indicate to which classification the various subjects belong. Measurement and random sampling are assigned to semantics; moments and product moments, and various types of inference, whether statistical, experimental, and empirical, are allotted to pragmatics; and the remaining topics to syntactics. However, one topic—' frequency classifications '—which concludes the volume, is left unclassified, since it is said to deal with both syntactical and pragmatical issues. In point of fact, it would not be difficult to show that quite half the chapters involve all three types of linguistic approach, for most of them include the designation of certain signs (semantics), a discussion of their interrelations (syntactics), and suggestions as to their interpretation (pragmatics). It is, no doubt, a praiseworthy aim to interest the research student in the modern theory of language and its uses; but it seems doubtful whether the beginning of an elementary course in statistics is the appropriate place. A. J. MORTON

Experimental Foundations of Clinical Psychology. Edited by ARTHUR J. BACHRACH. New York: Basic Books Inc., 1962. Pp. 641. $10.00.

Clinical psychology may be defined as the application of psychology to clinical problems, i.e., to the diagnosis, treatment and prevention of mental disorder'. Like all applied sciences, it is rooted in a body of knowledge which include both theory and practice. This body of knowledge has many parts. The most distinctive is psychometrics; but there are of course many others. The book edited by Professor Bachrach is concerned more especially with the relations between clinical psychology and these various parts or branches. The twenty contributors have done much more than demonstrate the nature and extent of the links. Each chapter provides a remarkably up-to-date and informative review of research in the special field with which it deals, and an adequate account of the relevant theories. The quality of the individual chapters, and the care that has been taken to avoid repetition and overlap, deserve high praise.

One point calls for special notice—the space devoted, not to clinical problems as such, but to material that is relatively remote, e.g., to the results of experiments on animals. This disproportion seems to reflect the primitive state of experimental clinical psychology (as distinct from general psychological experiments and observations on patients). But it may prove a source of comfort to the clinical psychologist who wishes to get away from routine clinical testing. It will furnish him with numerous ideas for research, and will show him that the whole field still lies wide open. In any case, the book is to be heartily recommended, with just a word of warning: it is impossible to dip into it without having one's attention immediately seized and so being forced to read it with care and interest.

MAX HAMILTON

Handbook of Abnormal Psychology. Edited by H. J. EYSENCK. London: Pitman Medical Publishing Co., 1960. Pp. xvi + 816. 120*s*.

This volume is in itself a monument of industry and enterprise. Of late, material dealing with the subjects covered, we are told, " has accumulated at the rate of a thousand articles and fifty books a year; and much of it has so far failed to penetrate into the text-books ". The purpose of the present work therefore has been " to gather together all these widely separated facts and theories into a new textbook compiled on a factual basis ". Such an undertaking has only become feasible through the enthusiastic collaboration of fifteen other psychologists, who at one time or another have been members of Professor Eysenck's Department, and the further help of " one or two colleagues from the psychiatric staff of the Maudsley and Bethlem Royal Hospitals ". The result is a valuable compendium of information, admirably indexed, which might well lie at the elbow of everyone who works in the same or similar fields.

In his introduction Professor Eysenck carefully defines the terms of his title. The word ' psychology ' is used, he says, " to denote the scientific study of the behaviour of organisms ". The word ' abnormal ' has hitherto been equated either (i) with what is 'statistically unusual' or (ii) with what 'deviates from the ideal norm', while (iii) in psychiatric writings it commonly refers to 'certain unusual and maladaptive states'. Professor Eysenck regards the third interpretation as untenable, and seeks to combine the first two. "Mental abnormalities", he contends, "do not represent qualitative disease entities having specific causes". And for this reason he "rejects the psychiatric framework outright, preferring not to be constrained by diagnostic schemes of dubious validity and known unreliability". Instead he and his coworkers "have tried to use the laws and hypotheses of general experimental psychology to account for the facts presented". Thus, throughout the book there is a stress on "integration through theory". In almost every chapter the value of quantitative methods and statistical techniques is clearly demonstrated; and Dr. Eysenck himself quotes Roger Bacon's dictum: "Physicians must know their science is impotent if they do not apply the power of mathematics."

The third term in the title is perhaps more open to question. Can a book weighing nearly four pounds be fairly described as a 'handbook' In German no doubt it would be described as a *Handbuch*; and, as the editor disarmingly remarks: "handbook is as handbook does".

The whole work is divided into three main parts. The first considers the description and measurement of abnormal behaviour in terms of sensori-motor functions, perceptual functions, thought processes, and 'ego functions'. The second deals with the 'causes and determinants' of abnormal behaviour—an omnibus phrase which is used not only to include genetic factors (admirably treated by Dr. Eliot Slater and Dr. Shields) and constitutional factors (discussed by Dr. L. Rees), but also covers conditioning, learning, somatic reactivity, and environmental conditions during childhood and later life (topics shared between five other contributors). Finally, part III deals with 'the experimental study and modification of abnormal behaviour'. Here a somewhat unusual but distinctly illuminating chapter by Dr. Broadhurst on abnormal behaviour in animals deserves particular attention. Dr. Meyer, Dr. Campbell, and Dr. Willett deal with the effects of brain damage, of electric-shock, and of psycho-surgical procedures. And towards the close Professor Eysenck himself reappears with two close-packed chapters on 'the effects of drugs' and 'the results of psychotherapy'.

Undoubtedly the attitude which the writers have adopted towards their subject, and many of the more specific views expressed, will evoke considerable criticism, particularly from those who have themselves been so frankly criticized. But this Dr. Eysenck himself will heartily welcome. "It is through criticism and counter-criticism that a growing science is advanced."

CYRIL BURT

Experiments in Personality. Edited by H. J. EYSENCK. London: Routledge & Kegan Paul, 1960. Vol. I, pp. xii + 262, Vol. II, pp. viii + 333. 40*s*. each.

The two volumes which make up this publication are subdivided into five parts. 'Part One' is entitled 'Experiments in Psychogenetics' and is contributed by P. L. Broadhurst. By way of a subtitle he describes it as 'applications of biometrical genetics to the inheritance of behaviour'. He points out, what few psychologists seem to have realized, that "modern genetics has developed out of the foundation laid by Fisher in 1918" by shaping and fitting together the biometric approach advocated by Pearson and Weldon with the Mendelian approach championed by Bateson and Morgan; and he goes on to quote Burt's review of the subject and his application of Fisher's methods to problems of mental inheritance. Broadhurst's own work, as reported here, is concerned chiefly with 'the psychogenetics of some aspects of emotional behaviour in the rat'. The planning of the whole series of experiments as well as the results obtained, deserve the special attention of psychologists interested in such questions.

'Part Two', by half a dozen different contributors, is concerned with 'experiments in psychopharmacology', and discusses the effects of depressant drugs on learning and conditioning, on perceptual processes, on vigilance and psychomotor performance, and on reaction times and skin resistance; and concludes with a suggestive paper by Professor and Mrs. Eysenck, describing a 'new method for the classification of drugs according to their behavioural effects'.

Volume II has for its subtitle 'Psychodiagnostics and Psychodynamics'. The former heading refers to an experimental and factorial study of 'thought disorder' in psychotic patients, leading to the suggestion that the 'schizophrenic group' can be divided into two sub-classes—one characterized chiefly by what Cameron has termed 'over-inclusive' thinking, and the other chiefly by retardation. The part dealing with 'psychodynamics' consists of four papers by various authors discussing the 'excitation-inhibition balance' in normals and neurotics, measures of learning and conditioning, and various perceptual functions, together with a factor analysis of the tests employed.

The volume, however, also includes a third part, describing 'experiments in psychometrics'. It consists of three papers by Patrick Slater, each of which will be of special interest to statistical psychologists. They deal with 'factor analysis and allied procedures', the 'canonical analysis of discriminance', and a re-examination of Hildebrand's data relating to the distinctions (i) between introverts and extraverts and (ii) between the normal and the neurotic, and intended primarily to confirm the view that these categories form two independent dimensions of personality, orthogonal to one another.

The book concludes with an illuminating 'Epilogue' by Professor Eysenck on the 'place of theory in psychology'. His final contentions are summed up as follows. In the study of human personality, "so long as we are dealing with a 'strong theory', successful predictions do not do much to enhance the validity of the theory, while failures are very serious, and may be disastrous; when we are dealing with a 'weak theory', the reverse is true: successful predictions, particularly if unlikely on common sense grounds, are then important and valuable, whereas failures are not unequivocal enough to be taken seriously". Professor Eysenck tells us that the book may be regarded as "a kind of *Festschrift*", reporting the results of the investigations undertaken during the ten years that have elapsed since the Institute of Psychiatry was founded as part of the University of London. It is certainly an excellent testimony to the wide programme of research that has been initiated and in part carried out.

G. R. ROBERTS

Know Your Own IQ. By H. J. EYSENCK. Harmondsworth: Penguin Books Ltd., 1962. Pp. 192. 3*s.* 6*d.*

This book has already evoked considerable criticism both from educationists and from psychologists. The educationists deplore the encouragement which they suppose it offers to parents and others who are only too eager to coach their children in devices of intelligence-testing in the hope of squeezing them into schools or occupations for which they may scarcely be fitted. The psychologists protest against the idea that an untrained reader can determine his own I.Q. by answering questions like those contained in this and similar publications. Both criticisms can perhaps be pressed too far; and both have been to some extent met by Professor Eysenck himself. He warns his readers, with all the emphasis that italics can bestow, that they "should not base any decisions on the results". "There is", he insists, "no reason why one should not take one's own temperature; but there is every reason why in the absence of a medical training one should not diagnose one's own illnesses on the basis of a thermometer reading". As for attempts at coaching, research has shown that their effects are comparatively slight; and, as Professor Eysenck observes, preliminary efforts with such problems as these—or the analogous exercises given in television programmes and other handbooks—tend to acquaint the youthful population with the nature of the ordeal, and so remove much of the anxiety and distress such examinations are apt to inspire. I would add that it is an excellent plan to acquaint the general public in this fashion with the essential notions of mental testing and to present them with an interesting and intelligible account of its aims, its methods, and its chief results. Professor Eysenck, for example, quotes in full Burt's findings for the I.Q.s of different social classes which show how the average adult from the professional class scores nearly 50 I.Q. points more than the unskilled labourer, and at the same time reveal a surprisingly wide distribution of individuals within each class, so that the ablest children from the lowest social class turn out to be far brighter than the dullest from the higher classes—facts of which the ordinary man and indeed most politicians are wholly unaware. And he then goes on to detail the various lines of evidence which indicate that differences such as these are to a large extent the effect of genetic constitution: " 80 per cent of the factors contributing to individual differences of intelligence are hereditary ". These are just the points that need driving home, and should do much to answer the questions and complaints that are so often raised.

Here and there, however, it must be owned, Professor Eysenck seems to play into the hands of his critics. He starts by telling us that " intelligence tests are not based on any very sound scientific principles ". Torn from its context, such words might easily supply an argument to the popular opponents whom Professor Eysenck so skilfully deflates later on. In point of fact, a vast amount of research has been devoted to determining the scientific principles on which every efficient and well standardized test should be based. Practically every psychologist working in this field is agreed that cognitive activities are all correlated positively with one another, and that the amount of correlation increases with the complexity of the activity tested; and it is a well established mathematical theorem that such a system of positive correlations can always be expressed in terms of a ' general factor '. First hand evidence, however, is needed to demonstrate that this ' general factor ' is to a large extent determined by a genetic component; and this is explicitly accepted and summarized by Professor Eysenck himself in a later portion of his book. If, as he assures us, " intelligence testing is very successful in its applications ", that is precisely *because* a " firm scientific basis " has been laid, not only in general theory, but also in the preliminary research into the reliability, validity, and item-analysis—a type of inquiry to which every battery of tests is, or should be, first subjected.

Unfortunately, as one commentator has already remarked, " Dr. Eysenck also appears to lend countenance to the impression that psychological testing is a foreign importation into this country ". It was, he says, " the French psychologist Binet who came up with the notion that mental abilities should be measured by means of mental tests ". Actually it was a British psychologist, Sir Francis Galton, who first " came up with the notion "; and Binet is quite frank in acknowledging his debt. Again we are told that it was " work in the United States during the First World War " which " led directly from the use of individually administered tests of the Binet type to group tests, such as the one presented in this book ". But in point of fact the merits—and the limitations—of group tests, as contrasted with those of the Binet type, were originally demonstrated by British investigators, long before the outbreak of the First World War. By 1914 they were already in use for large scale surveys by British education authorities. Finally, it seems a pity that, to illustrate the predictive value of intelligence tests, the only detailed evidence offered relates, not to follow-up studies of British school children or of occupational applicants, but to results obtained with the U.S. Army General Classification Test and the test battery employed in selecting pilots in the U.S. Air Force. Perhaps in a future edition the results and the evidence furnished by research in this country could be briefly summarized as well.

The bulk of the book consists of eight half-hour tests, each containing forty questions. These are undoubtedly highly entertaining in themselves. To the psychologist their chief interest lies in the fact that to a large extent they succeed in breaking away from the stock types of problem—syllogisms, reasoning tests, completion tests, and such hardy annuals as opposites, synonyms, analogies, etc.—or at any rate in presenting them in an entirely novel guise. The attempt, however, has at times been made at the cost of genuine efficiency. A few trials will quickly show that in certain cases the most intelligent persons do not give the somewhat far-fetched answers that the key rather surprisingly enjoins. In item 34 of Test 7, for instance, we are asked to " underline the odd-man-out in the following series: animal, engine, identity, octagon, unicorn ". Now in my own experience the most intelligent examinees underline ' identity ', because it is the only abstract word. According to the key, however, the first answer is wrong: the right solution is ' unicorn ', " because in all the other words the first and last vowels are identical ". But this is not strictly correct, since the last vowel in ' identity ' is y not i. If we are to go by letters not by meanings, surely it would be better to choose ' octagon ' as the only word which does not contain the letter i. The same objection holds against item 33 of Test 2: " underline the odd-man-out " among " courage, leave, measles, steamer". The intelligent underline ' leave ', because it is the only verb; the key requires ' steamer ', because it is the only word that is not " colloquially connected with a nationality—Dutch courage, French leave, German measles "; but surely one can talk of a ' German steamer ' just as readily as of ' German measles '. Many of the tests depend on specialized knowledge: e.g., " which town is the odd man out " among " Oslo, London, New York, Cairo, Bombay, Caracas, Madrid? " Correct answer—' Madrid ', because " each of the others lies approximately 10 degrees south of the preceding". Other tests assume a familiarity with such things as the names of Greek philosophers, musical composers, capital cities, and so forth. Then again, for many of the diagrammatical and numerical tests alternative solutions, often much simpler than those necessitated by the key, can readily be found: e.g., the examinee is asked to " insert the missing number " in the following matrix:

" 4 8 20
9 3 15
6 6 ".

The obvious solution is that the figure in the third column is obtained by adding to the figure in the first column twice the figure in the second. This gives the simplest answer, $6+2\times 6=18$. According to the key we must mark this as wrong: the correct answer, we are told, is 24: the explanation is that to get the figure in the third column we must " subtract the *sum* of the numbers in the first two columns from their *product* ".

Most of these objections could easily be dispelled by a few slight changes when revising the book for a future edition. They are objections too that are likely to weigh more with the lay reader than with the psychological specialist. The psychologist realizes that the proper way to judge a test is by the statistical data obtained to demonstrate its efficiency, not by mere armchair introspection. But here again the book is disappointing. What the psychologist would like to know is the actual evidence showing that these novel types of test are at least as effective as the older types. How many people have been tested with them in the preliminary studies of reliability, validity, and item-analysis, and what precisely were the results? How were the marks standardized in terms of an I.Q.? A very rough trial suggests that many of the items are exceptionally difficult. Indeed, we actually learn that, in general, to score half the total marks you must have an I.Q. of 135 or thereabouts, which means you must be among the top 1 per cent of the population. I wonder how many readers will score, shall we say, three-quarters of the total number.

It is therefore not surprising to find that a good many psychologists have been inclined to criticize the project as a whole. They not unnaturally fear that, in spite of the incidental warnings, a large number of uninstructed readers will suppose from the title and the author's name that here they have a scientific method of assessing their own intelligence, and, when they fail, will either conclude that they are hopelessly incompetent or alternatively that the whole technique of mental testing is devoid of any sound basis. G. R. Roberts

Educating Older People. By M. F. Cleugh. London: Tavistock Publications, 1962. Pp. 185. 28*s*.

Dr. Cleugh is Senior Lecturer at the Institute of Education in the University of London; and the views which she sets out in her latest book are based on more than a dozen years' experience in tutoring adult students. As she explains in her introduction, she is not concerned with " adult education as the phrase is usually interpreted ". Her aim is rather " to consider how the educative process works when its subjects are not young people but older persons, not unwilling victims but voluntary contractors, not green saplings but sturdy and unbending timber—in short, adults with a formed outlook on life ". She discusses in successive chapters ' how to get things going ', the problems of lecturing, of discussions, of supervising practical work, of examinations, and finally the change of attitudes and relations. Almost every page is rich with first hand observations and examples; and the whole volume is written in a lively and delightful style. Every university and college teacher should study it.

E. L. Ridley

Society: Problems and Methods of Study. Edited by A. T. Welford, M. Argyle, D. V. Glass, and J. N. Morris. London: Routledge & Kegan Paul, 1962. Pp. vi + 586. 50*s*.

" The present volume ", so the editors explain, " is intended for those setting out upon a systematic study of social processes and for those whose main interest lies elsewhere but who wish to orient themselves in particular parts of the field "; and in selecting both problems and authors they " have concentrated more particularly on recent British thought and research ". There are four editors and thirty-four contributors. Why so many editors were needed is something of a puzzle, for they seem to have done very little work. They contribute a joint ' introduction ' of less than a page; and throughout the volume it is obvious that " the editorial hand " (as they put it) " has been kept light ", and that " inevitably the reader will find gaps ". It is a pity therefore that the student who is " setting out on a systematic study " is not informed in advance what is the ground which the book was designed to cover, and what exactly are the " gaps "—some of which (we are told) " have been deliberate ". After reading the volume one is inclined to conclude that their guiding policy was to concentrate mainly on what is sometimes called ' applied sociology '. Hence the most obvious gaps relate to theory. Two of the authors—those responsible for the opening chapter—describe the work as a fresh edition of *The Study of Society: Methods and Problems* (published in 1939). That also had four editors. But the present editors make no reference to this ' previous edition '; and indeed the choice of methods and problems has been changed considerably—not always for the better. Here, in discussing the contents, it will be possible only to touch on points likely to be of interest to statistical psychologists.

The volume is divided into two main parts. The first is entitled 'Approaches and Methods of Study '. Here the chapters on ' Statistical Surveys ' (by C. A. Moser and E. Grebenik) and on ' The Population Census as a Source of Social Statistics ' (by B. Benjamin) are admirable, and deserve the attention of every psychological research-student. In addition to the " methods commonly associated with social study " an attempt has been made to include " others, such as ' operational research ', which seem likely to . . . make an increasing contribution to the understanding of society ". Accordingly we not only find the ' method ' of operational research discussed in the first part with a chapter to itself, but encounter later chapters on ' operational research in industry ' and ' an operational approach to criminology '. It is, however, by no means easy to discover what precisely the editors meant us to understand by this new ' method '. One writer tells us that operational research is ' a markedly sociological science '; another that it is ' akin to a movement rather than a discipline '. As a matter of fact, the phrase became popular, and useful, during the recent war; and from the journal recently established to deal with the subject—

'The Operational Research Quarterly'—we gather that the term has altered its meaning from time to time, but is now commonly used to denote "the application of scientific methods of research to the investigation, not of the theoretical problems of pure science, but of the practical problems that face administrative and executive authorities". In short, it is merely a high sounding phrase to designate what was formerly called 'applied science'—or at least a certain branch of it. Whether this is the meaning implicitly adopted by the present writers we are not informed.

The second part of the volume is entitled 'Problems and Applications'. These are classified under four headings—industry, social pathology, population studies, and aspects of sociology. In this section what will chiefly interest readers of this journal are the various discussions relating to differences in intelligence and its effects on class structure, educational achievement, occupational advancement, and the future welfare of the nation.

James Maxwell examines the problem of the alleged decline in national intelligence. He begins by summarizing the main results of the two Scottish surveys. "In the *individual* tests", it will be remembered, "the average I.Q.s of the 1932 and 1947 samples were not significantly different" (dropping from 99·21 to 99·18); but "on the *group* test the 1947 children scored significantly higher" (rising from 34·46 to 37·74 marks). As Dr. Maxwell points out, this agrees with the experience elsewhere. "There is," as he rightly says, "fairly clear evidence that familiarity with group intelligence tests can raise children's I.Q.s as much as 5 points; . . . and where, as in England, verbal groups are extensively used, the standard of children's performance in them has risen." It follows that no safe inferences can as yet be drawn regarding the change, or lack of change, in the *innate* intelligence of the nation, particularly as "an interval of only half a generation is far from adequate for any genetic change (unless very drastic) to manifest itself". Indeed, as Burt and his co-workers have pointed out in describing successive surveys carried out in London schools, there has been during the past fifty years a steady improvement in the environmental conditions affecting the health and well-being of the mass of children who attend public elementary schools—particularly the girls, and consequently it is quite conceivable that a resultant acceleration of growth during childhood might actually mask a slight decline in the level of innate ability.

Dr. Lockwood contributes an instructive review of recent researches on social mobility. Unfortunately the most elaborate sociological study of the problem—that carried out by David Glass and his colleagues—relates to mobility in the period preceding the Education Act of 1944, and is therefore now somewhat out of date. Dr. Lockwood cites some of the later researches reported in this journal; but there is an urgent need to apply for fresh inquiries with up-to-date procedures to determine how far the position has changed. From the data available it would appear as though the change has been much smaller than was expected or hoped for. It is true that a far larger number of working-class children now enter grammar schools, nevertheless a considerable proportion, we are told, "end up by doing badly towards the end of the course or by leaving before completing it". So that in the end, it would seem, their situation remains much the same as if they had never entered. If so, it may turn out that the effects of the recent educational reform on social mobility has been far less than was predicted or is often assumed.

Dr. Lockwood's paper on mobility is followed by one from Dr. Jean Floud on 'The Sociology of Education', which is likewise illustrated by statistical tables and turns partly on statistical inferences. In her view "the problems that give rise to, or at least justify, the sociology of education are created by industrialism". "British sociologists", she explains, "have been drawn to study education through their interest in problems of social stratification and mobility." Before the sociologists came to the rescue, we gather, the work on such problems carried out by psychologists and educational investigators was "bedevilled by their preoccupation with heredity and environment". "In the thirties", however, "the limitations of available measures of ability—'Intelligence Tests'—were carefully explored." The exploration alluded to (so the references inform us) was contained in three successive publications by a single London sociologist. As it happens, however, his choice of intelligence tests was drastically criticized at the time by educational psychologists of experience, though possibly Dr. Floud's own memory does not go back so far as this. In the course of his exploration, we are told, it was discovered that " the measuring of innate intelligence appears to be impossible at present or in the foreseeable future " and so " the question of genetic limits to educability has to be by-passed ". Dr. Floud quotes with approval the curious view that " once a person has a certain minimal level of intelligence, his performance beyond that point is uncorrelated with his ability ". This would seem to imply that, provided a child is not actually mentally deficient, the teacher should find no difficulty in teaching him the differential calculus, and that it is only the handicaps of the traditional class-structure that prevent every normal child from becoming a First Class Classical Scholar at Oxford or Senior Wrangler at Cambridge!

Dr. Floud goes on to talk of the analysis of " environmental influences on educational performance, as measured by standardized tests of ' intelligence ' ". Such phrases, however, may gravely mislead the student who is " setting out upon the study ", for he cannot be expected to know that the explicit aim of those who devised these ' standardized tests of intelligence ' was to assess it *independently* of environmental influences: to measure educational performance both Binet and Burt developed a completely separate set of tests. What is more, the introduction of tests of intelligence unquestionably saved many children, who had been gravely handicapped by ' environmental influences ', from being certified as mentally deficient, and enabled many others to win scholarships at secondary schools. In spite of recent educational and social reforms " the influence of crude material handicaps ", says Dr. Floud, " can still be traced. But in a prosperous area the influence of the home is more subtle. It has been shown that children securing entrance to a grammar school . . . are those whose parents are relatively better educated and more ambitious for their educational future ". Apparently she does not realize that, if the parents as well as the children are " relatively better educated " and set a higher store by intellectual ambitions as distinct from economic or material gain, then these facts of themselves provide presumptive evidence of a superior intellectual and temperamental endowment inherited by the family. Here, as in several other contributions, we encounter one of the most conspicuous gaps characteristic of the book as a whole. The importance of the *biological* background to social problems, which was brought out so clearly by Sir Ronald Fisher and his school, and is still strongly defended by geneticists like Professor Darlington, is almost entirely ignored—or, as Dr. Floud seems to imply, deliberately " by-passed ". Naturally as sociologists the contributors are chiefly interested in social factors. But why should this exclude the possible influence of genetic factors? And why is it necessary for genetic questions to be passed by?

As a psychologist therefore I am tempted to register an incidental protest against the way in which terms, concepts, and conclusions falling within the psychologists' province are repeatedly misrepresented. Elsewhere in the volume the reader is categorically assured that " there is no identifiable psychological entity called ' intelligence ', though there is substantial agreement about the characteristics of intelligent behaviour ", and again that " there is no uniformly accepted convention expressing the distribution of I.Q.s among the population ". But these are matters on which the psychologist, not the sociologist, should have the last word. And those psychologists who have adopted the word ' intelligence ' as a scientific term have *not* used it to describe certain types of behaviour; that is merely the popular usage. They have taken it as a convenient technical label to denote the innate component of the general factor underlying all cognitive abilities: (' entity ' is hardly the word a psychologist would employ in such a context). This definition, with trifling modifications in wording, has been put forward again and again; and, even if a few sociologists remain unconvinced by the evidence on which it rests, it seems hardly fair to " those setting out on a systematic study of social processes " if the definition and the evidence are passed over in silence.

Again there *is* an " accepted convention expressing the distribution of I.Q.s, namely, that which assumes that the distribution is approximately normal with a standard deviation of 15 points. The convention may not be " *uniformly* accepted "; but is *any* scientific convention accepted with absolute uniformity? Nor would any psychologist deny that the factor of innate intelligence cannot be directly measured apart from other factors, some of which are doubtless ' social factors '; yet a fairly trustworthy *estimate* can commonly be derived. For convenience such estimates may be expressed in terms of an I.Q.; but not every I.Q. can claim to be a reliable estimate of innate ability. To talk of '*the* I.Q.' of a particular individual, and to point out that after an appreciable interval it may reveal an increase varying largely on the degree of education, does not demonstrate that *every* estimate is bound to be so affected. Dr. Floud and those who think with her are presumably referring to the unchecked and uncorrected marks obtained with verbal group tests, such as those which are used in the 11 plus examination; and, of course, the uninstructed student will think the same. But in a serious textbook the authors should point out, as psychologists have so frequently done, that these rough assessments are *not* the kind of estimates on which the research-worker relies in a scientific inquiry.

However, the minor blemishes thus discernible in several of the contributions should not outweigh the merits of the rest. Whether or not the psychologist agrees with the authors on this or that particular point, their work as a whole undoubtedly deserves his serious study and attention.

T. F. Miller

ERRATA

Professor Cureton sends the following corrections to Table II in his ' Note on Simultaneous Equations and Matrix Inversion ' (this *Journal*, XV, 1962, p. 53).

(i) In section A the terms on the right in the third, sixth, and ninth equations should be joined by positive signs, not negative. Thus the third equation should read

$$a_{ij\cdot 1} = a_{ij} + a_{i1}\, u_{1j}.$$

(ii) In section C the first term of the first equation should *not* have the negative sign; it should read

$$r^2_{m1} = a_{10}u_{10} - a_{20\cdot 1}u_{20} - a_{30\cdot 2}u_{30} - a_{40\cdot 3}u_{40};$$

a_{10} already has a negative sign in the basic layout.

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OCCUPATIONAL PSYCHOLOGY

Editor : ALEC RODGER

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